School Food Policy Affects Everyone: Retail Responses to the National School Lunch Program*

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Abstract

We study the private market response to the National School Lunch Program, documenting economically meaningful spillovers to non-recipients. We focus on the Community Eligibility Provision (CEP), an expansion of the lunch program under the 2010 Healthy, Hunger-Free Kids Act. Under the CEP, participating schools offer free lunch to all students. We leverage both the staggered roll-out and eligibility criterion of the CEP, which is limited to schools where at least 40% of students participate in other means-tested welfare programs. We find that local adoption of CEP leads to a 10% decline in grocery sales at large retail chains. Retailers respond with chain-level price adjustments: chains with the most exposure lower prices by 2.5% across all outlets in the years following adoption, so that the program's welfare benefits propagate spatially. Using a stylized model of grocery demand, we estimate that by 2016 the indirect benefit had reduced grocery costs for the median household by approximately 4.5%.

JEL Codes: H42, I38, L11, R32

1 Introduction

Food security programs are integral to the U.S. social safety net, exceeding $90 billion in 2019. Naturally, a large literature in economics focuses on measuring the welfare benefits

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these programs provide to their target populations, who typically number among the most vulnerable members of society. The sheer magnitude of these programs suggests that they might also elicit private sector responses that affect the general population. This paper presents new evidence on the impact of public programs on the private sector in the context of the National School Lunch Program (NSLP), which provides meals to children through schools, serving some 30.4 million children in 2016.[1,2]

The documented benefits of the NSLP for students are substantial, and include higher attendance and test scores (Ruffini (Forthcoming), Schwartz and Rothbart (2020), Frisvold (2015), Schanzenbach (2009), and Bhattacharya and Haider (2006)).We show that the NSLP also reduces household spending at supermarkets when children receive free meals at school. Grocery stores in turn adjust product prices. Importantly, and in line with recent descriptive evidence on uniform pricing in U.S. retail (DellaVigna and Gentzkow (2019), Adams and Williams (2019), Hitsch et al. (2019)), we find that price effects propagate through chains such that even communities with low program take-up benefit. The indirect benefits enjoyed by all households amount to approximately 10% of the direct benefit enjoyed by families receiving free lunch.

Our identification strategy exploits the Community Eligibility Provision (CEP), a national program expanding the NSLP under the Healthy, Hunger-Free Kids Act of 2010. Historically, schools collected lunch applications from families to verify individual student eligibility for free or reduced-price lunch. Schools that adopt the CEP need not collect applications, but rather serve free lunch to all students. The aim of the CEP is to reduce the administrative burden of the lunch program in high-poverty areas, where many students qualify for free lunch under older provisions. In practice, if at least 40% of a school's enrolled students are categorically-eligible for free school lunch, which means that the students qualify for other means-tested welfare programs, then the school qualifies for the CEP. To be clear, a school where 40% of students qualify for free lunch under certain older NSLP provisions may now offer all students free lunch–a 150% increase in the number of free-lunch eligible students. In the 2016-2017 Academic Year, 20,721 schools participated in the NSLP under the CEP, a combined 9.7 million students across 3,538 school districts (Food Research & Action Center, 2017).[3] Ruffini (Forthcoming) presents evidence that the program dramatically increased meals served at participating schools, on the order of 12% for lunches and 38% for breakfast (because the School Breakfast Program is smaller in scope, we focus primarily on the NSLP, although the CEP integrates similarly with both programs). Our first finding is that the program meaningfully reduces residual demand facing grocery retailers in the private market.

[1]For comparison, the largest domestic hunger safety net program, the Supplemental Nutrition Assistance Program (SNAP), gave an average of $125.40 on debit cards to some 44 million recipients in 2016. SNAP restricts allotment spending (e.g. alcohol and to tobacco do not quality), but is far less restrictive than the NSLP.

[2]USDA NSLP Fact Sheet, November 2017.

[3]Program take-up was elective. We exploit the staggered rollout and eligibility cutoff to deal with endogenous participation.

Following adoption, households with school-aged children take fewer trips to and spend less money at grocery stores, especially large retail chains. This finding is consistent with evidence from Schanzenbach and Zaki (2014), who show that many marginal students who participate in an experimental free school breakfast program would otherwise eat breakfast at home.

The paper then focuses on understanding how grocery stores respond to the CEP demand shock. We present a simple model of retailer profit maximization, where the CEP serves to reduce the price of a substitute product, the public option. If prices are strategic complements, then the retailer ought to lower the price of store-bought lunch in response to the CEP, generating consumer surplus for adults and children who do not benefit from the CEP directly. This prediction is consistent with Cunha et al. (2018), who find that the prices of grocery products fall following the institution of a food delivery program in rural Mexico. However, if prices are strategic substitutes, retailers might instead increase prices. Higher prices would dovetail with findings from the pharmaceutical industry, wherein many branded drugs increase prices when generics enter at low prices (Frank and Salkever, 1992). Whether prices are strategic complements or substitutes depends on whether a reduction in the price of school lunch increases or decreases the own-price demand elasticity for groceries. In theorizing about the impact of the CEP, we also incorporate an insight from the industrial organization literature: namely, that grocery retail chains often employ uniform pricing policies (DellaVigna and Gentzkow (2019), Hitsch et al. (2019), and Adams and Williams (2019)). The CEP presents an opportunity to test the uniform-pricing theory by examining the extent to which prices respond to local adoption of the CEP and/or chain-level exposure to the program. We note that the CEP may also affect upstream firms, such as distributors and wholesalers, but we leave analysis of these to future work.

A central concern in estimating the causal effect of the CEP demand shock on grocery retail is selection into the CEP. Because school participation is elective, schools that serve communities where children receive little nutrition at home may be the most likely to join, inducing a correlation between uptake, local grocery revenue, and local grocery prices. We employ two strategies to identify the causal effect of the CEP on grocery stores. First, we compare purchases of households with and without children before and after their local school(s) adopt the CEP. Because adults do not receive food through the NSLP, adult-only households serve as a control group in this comparison. The second strategy exploits two aspects of the program to mitigate endogeneity concerns: the discontinuity in school eligibility at 40% and the staggered roll-out of the program, which became available to different states between 2011 and 2014.

We find that grocery prices fall with chain exposure to the CEP. A one standard deviation increase in chain CEP exposure (roughly 8 percentage points) leads to a 2.5% price reduction across all stores in the chain. In contrast, we find no evidence that prices respond to local adoption of the CEP. To our knowledge, this paper is the first to provide empirical evidence that uniform pricing dampens price responses in areas where chain exposure is low, but local adoption is high (and vice versa). Recent work by García-Lembergman (2020) finds similar patterns in retailer price responses to housing market shocks during the Great Recession. One advantage of our setting is that it permits two checks on our empirical strategy that provide confidence in our conclusions: first, and in contrast with price effects, we show that store revenues fall with local rather than chain exposure to the CEP. Second, we show that these revenue declines are largest in categories with high family appeal.

Through the price effects that we document, policies like the NSLP impact the wider community beyond the direct recipients of free school meals. We estimate a stylized model of grocery demand to quantify the welfare benefits of these indirect effects. In the model, households that live near chains with high exposure to the CEP demand shock can benefit from the CEP—even if they do not have school aged children—through lower prices. The model could also be extended to incorporate extenstive margin responses to the CEP demand shock, although we do not find strong evidence for these in our reduced-form analysis. Our estimates imply that by 2016 the indirect effect of the CEP amounted to a 4.5% welfare enhancement for the median household. Our findings highlight a difference between in-kind programs like the NSLP and in-cash programs, which have been shown to harm bystander households (e.g., Filmer et al., 2021, Leung and Seo, 2018, and Hastings and Washington, 2010). In the case of U.S. retail, we note that the use of chain-level pricing facilitates the separate identification of price and distance elasticities, which might be useful in future work seeking to quantify grocery costs using an index that accounts for variation in both the proximity of stores and the prices they charge.[4]

The rest of the paper proceeds as follows. Section 2 details the Community Eligibility Provision and explores its incentives. Section 4 describes the data. Section 3 presents a simple model of firm pricing as a function of the price of the public lunch option. Section 5 contains our estimation strategy and results on revenues. Results on entry, exit, pricing, and assortment are presented in section 6. Section 7 quantifies the welfare impacts of price changes in local retail environments. Section 8 concludes.

2 Background on the National School Lunch Program

Since 1946, the U.S. Department of Agriculture has administered the National School Lunch Program (NSLP), which provides nutritious, low-cost meals to students in both public and not-for-profit schools. The program is large; it served some 30.4 million children in 2016. Participating schools receive reimbursements from the federal government for meals served to

[4] Eizenberg et al. (2021) also estimate a model of grocery store choice that allows for spatial frictions and price sensitivity. They use this model to study price competition. We instead use our estimates to measure changes in local grocery cost indexes, akin to those calculated in Ellickson et al. (2020) who assume prices are fixed and Atkin et al. (2018) who abstract from within-municipality spatial frictions.

children from low-income families. Schools may also receive food directly from the USDA. In return, school meals must meet certain nutritional requirements, and low-income students must receive free or reduced rates (40 cents). Paid rates, which are set locally, averaged \$2.63 in the 2016-2017 academic year.[5] Students can qualify for free or reduced price lunches "categorically" if they or their family participate in another means-tested welfare program, including the Supplemental Nutrition Assistance Program. Students can also qualify based on household income and family size, as follows: those below 130% (130%-185%) of the federal poverty line are eligible for free- (reduced-price) lunch.[6] To qualify for free or reduced-price lunch based on income, families must submit an application to their school or district. In the 2017-2018 academic year, reimbursements were on the order of \$3.31 per meal served to a free-lunch student, \$2.91 for a reduced-price student, and \$0.31 for a paid student.[7] The Community Eligibility Provision, described below, aims at reducing the administrative burden of providing free lunch for high-poverty schools (including application collection and processing).

The CEP is the fourth provision of the National School Lunch Program, rolled out at the state-level between 2011 and 2014 according to the schedule depicted in Table 1. The CEP provides participating schools partial reimbursement for meals served, in return for which it requires that participating schools offer all enrolled students free lunch, regardless of each student's individual financial circumstances.[8] Participating schools are reimbursed for meals served according to the school's Identified Student Percentage (ISP), the proportion of students who qualify for the NSLP categorically. The per-pupil reimbursement rate is proportional to 1.6 times the school's ISP. Any school with an ISP above 62.5% therefore receives the maximum per-pupil funding, while those below must fill the gap left from foregone paid lunch revenues with state or local funding. Central to our identification strategy presented in section 5, a school must have an ISP at or above 40% to qualify for the provision unilaterally. Schools can also participate as part of a Local Educational Agency (LEA). That is, a school that does not qualify individually can pool with higher-poverty schools, so long as their pooled ISP exceeds 40%.

[5] School Nutrition Association, "School Meal Trends & Stats," `https://schoolnutrition.org/aboutschoolmeals/schoolmealtrendsstats/`..

[6] The National School Lunch Program Fact Sheet, USDA, 2017

[7] Schools in Alaska, Hawaii, and Puerto Rico receive additional payments. Schools may also receive an addition 6 cents per pupil if they comply with additional USDA standards. See: Federal Register, July 2018, Vol 83, No 139.

[8] Through the CEP, schools can continue to provide meals to students in the summer, e.g., through the USDA Seamless Summer Option or the Summer Food Service Program. See `https://www.fns.usda.gov/sfsp/seamless-summer-and-other-options-schools`.

Table 1: Roll-out of the Community Eligibility Provision

Initial Participation Year	State
2011-2012	Illinois, Kentucky, Michigan
2012-2013	New York, Ohio, West Virginia, the District of Columbia
2013-2014	Georgia, Florida, Maryland, Massachusetts
2014-2015	Remaining States

Source: Segal et al. (2016).

3 How the CEP Might Affect Retail Pricing

In this section, we illustrate how CEP adoption might affect local grocers, beginning with the household decision of what to buy for lunch. Suppose that each household with school aged children chooses between school lunch at price p_S and grocery store-bought lunch at price p_G. (For simplicity, suppose that the supermarket sells only a single lunch product.) Let $q(p_G, p_S)$ represent the household's demand for grocery-bought lunch. The Community Eligibility Provision affects grocery demand because it lowers p_S for some households (those that do not qualify for free lunch under older provisions of the NSLP). Holding grocery prices fixed, the CEP reduces the sales of store-bought lunch so long as the cross-price elasticity is positive:

$$\frac{\partial q(p_G, p_S)}{\partial p_S} > 0.$$

To understand how the CEP affects supermarket pricing, we aggregate the demand for grocery-bought lunch for households with children ($q(p_G, p_S)$) with that for households without children denoting the sum $Q(p_G, p_S)$. Grocery store profits are then $\pi(p_G, p_S) = (p_G - c) \cdot Q(p_G, p_S)$ where c is a constant marginal cost. If grocery and school lunch prices are strategic complements (substitutes), then grocery stores ought to reduce (increase) prices in response to the CEP. Complementarity/substitutability depends on the sign of $\frac{\partial^2 \pi}{\partial p_G \partial p_S}$. The first derivative of store profit with respect to own-price gives the familiar first order condition for a Nash Equilibrium in prices:

$$\frac{\partial \pi}{\partial p_G} = Q(p_G, p_S) + (p_G - c) \cdot \frac{\partial Q(p_G, p_S)}{\partial p_G} = 0. \quad (1)$$

The cross-derivative is then:

$$\frac{\partial^2 \pi}{\partial p_G \partial p_S} = \frac{\partial Q(p_G, p_S)}{\partial p_S} + (p_G - c) \cdot \frac{\partial^2 Q(p_G, p_S)}{\partial p_G \partial p_S}.$$

We expect that the first term is positive, which is to say that as the price of school lunch falls, fewer households will purchase store-bought lunch (per the positive cross-price elasticity assumed above). Therefore, prices are strategic complements unless the cross-partial $\frac{\partial^2 Q(p_G,p_S)}{\partial p_G \partial p_S}$ is large and negative. In standard demand models such as the logit, demand becomes more elastic as the market share of a product falls, but we could see the opposite if a decline in the price of school lunch leaves only the wealthiest (demand inelastic) families shopping for lunch at grocery stores. Thus, we turn to empirical analysis to understand how grocers respond to the CEP in practice.

This intuition carries over to a model that incorporates uniform pricing. Recent evidence suggests that retail outlets do not price according to equation (1), but instead that chain firms set uniform prices to maximize total profits across all outlets. In the CEP context, prices then ought to respond to the retail chain's overall exposure to the program. From the chain's perspective, the CEP reduces the average price of school lunch $\bar{p}_S$ in proportion to program take-up across markets where the chain competes. Uniform pricing implies that prices at some outlets may respond to the CEP even if its nearby schools do not participate in the program. We test for a uniform-pricing response in the empirical analysis below.

4 Data

Below we describe the three datasets used in our main analysis: school-level CEP participation and eligibility, purchases and demographics of nearby households, and sales of nearby stores. We complement these data with tract-level demographic data from the American Community Survey (ACS).

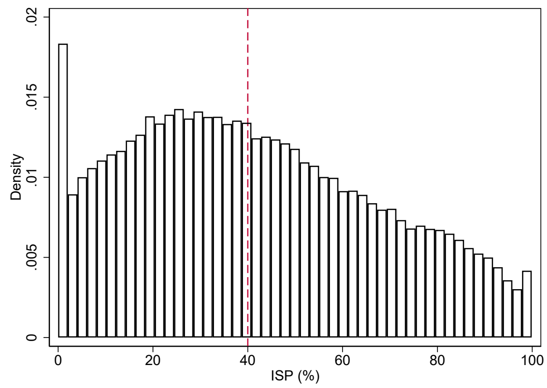
4.1 Program Participation and Eligibility

Our primary data on CEP participation comes from the National Center for Education Statistics (NCES). This data spans three academic years: 2013/2014, 2014/2015, and 2015/2016. The NCES contains 70,555 schools across fifty states. We collect earlier participation data from the seven states that adopted the program in 2011/2012 or 2012/2013 from websites and FOIA requests of the individual state Departments of Education.[9] We note that the number of participating NCES schools is approximately 30% lower than the figure reported by the Center for Budget & Policy Priorities.[10] In section 6, we adjust for underreporting when interpreting our results.

[9] For all states, we attempted to gather school-level data on the Identified Student Percentage (ISP), which is used to determine school eligibility for the program. Unfortunately, this data is not available from the early years of the program. Further, districts were required to report ISPs only for eligible schools initially, truncating the distribution of observed ISPs from below.

[10] The CBPP complies participation from lists published by each state for 2015/2016 and 2016/2017, as described in https://www.cbpp.org/research/food-assistance/community-eligibility-adoption-rises-for-the-2015-2016-school-year.

Figure 1: FRL from the NCES Data Set for the 2009-2010 School Year

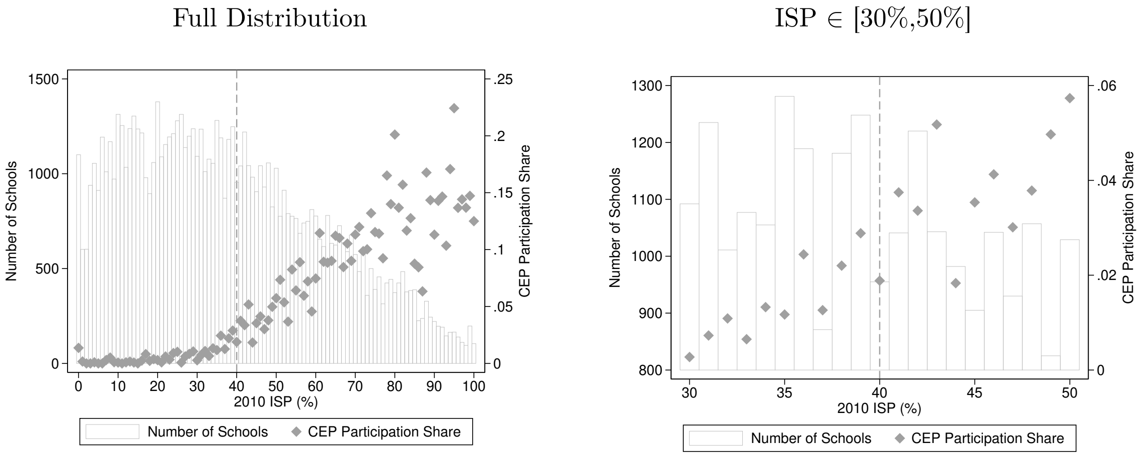


Notes: This figure plots the distribution of the share of students eligible for free and reduced price lunches (FRL) in the 2009-2010 AY. We employ FRL as a proxy for the identified student percentage, ISP, which governs school eligibility for participation in the Community Eligibility Provision.

To measure individual school eligibility for the program, we collect school-level student enrollment and free lunch eligibility rates for academic years 2009/2010 through 2015/2016 from the NCES. We use the fraction of students who qualify for free and reduced-price lunch as a proxy for each school's identified student percentage (ISP). This measure overestimates the true ISP because it includes students who qualify both through direct certification and family income.[11] It is therefore a generous measure of individual school eligibility. We favor 2010 ISPs because in the following year, some schools adopt the CEP, and thus may report that 100% of their students are free-lunch eligible (as all students do indeed receive free lunches). Further, because CEP schools do not collect lunch applications, there is no record of which students would have successfully applied under the income requirements. Figure 1 shows the distribution of ISPs across schools in 2009-2010, and does not reveal any perceptible bunching above the 40% cutoff. We formally test for manipulation using the methodology in McCrary (2008), and cannot reject a null hypothesis of no break in the density of ISPs at 40% (the discontinuity estimate is 0.030 with a standard error 0.024). For simplicity, throughout the rest of the paper, we refer to this measure as ISP.

[11] A student may be directly certified if their family receives SNAP, FDPIR, TANF; if the student is enrolled in a Head Start program; if the student is homeless, runaway, migrant, or a foster child. (USDA, *Community Eligibility: Planning and Implementation Guidance,* January 2016.)

Figure 2: School CEP Adoption Share



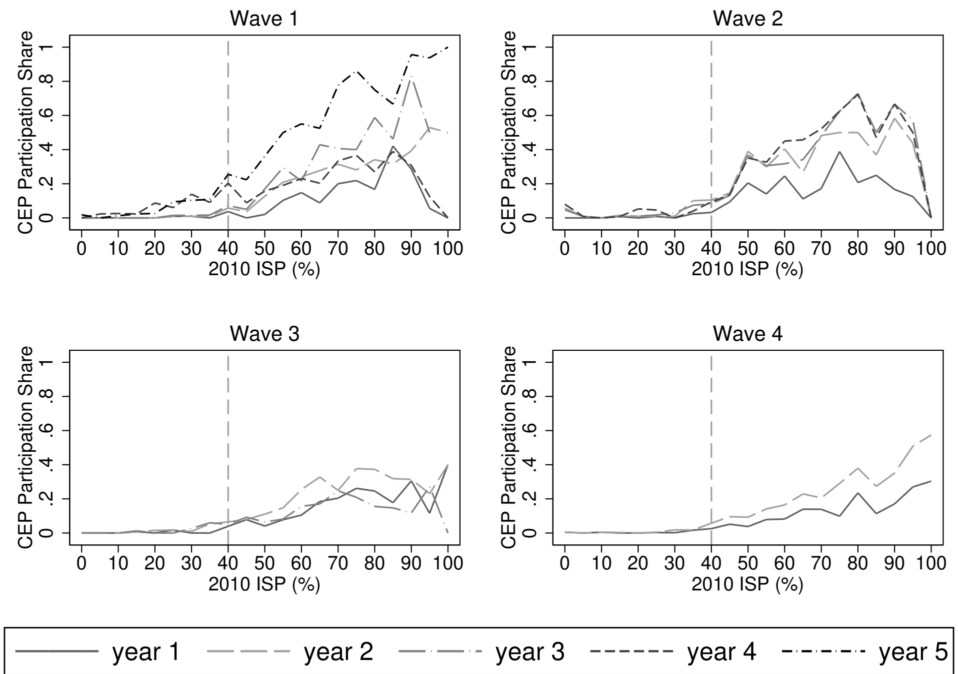
Notes: Each point reflects the share of schools within a given percentile of ISP that participate in the school lunch program. The right-hand plot focuses in on the ISP bins between 30 and 50.

Figure 2 plots average participation rates for schools at each percentile of ISP for the 2014-2015 Academic Year. Participation rates hover right around zero until 30%, and then begin to slope upward. Visual inspection does not suggest a discontinuous increase in participation in the CEP right at 40%. There are two possible reasons that the participation rate is smooth at the 40% ISP cutoff. First, using a proxy for ISP ought to smooth any jump at the threshold (Pei and Shen, 2017); and second, schools with ISPs below the threshold may participate if they do so as part of a group of schools where the pooled ISP exceeds 40%. For the state of Wisconsin, we collected actual ISP data as well as information on whether schools participate in the CEP individually or as part of a group in the 2016-2017 school year. In this data, there is a sharp discontinuity in individual-school CEP participation at 40%, and no schools with an ISP below 40% participate in the program individually. In appendix section B.1, we use these Wisconsin data to implement a fuzzy regression discontinuity design to study the response of lunches served to the CEP in Wisconsin.

Our chief identification strategy involves a difference-in-difference methodology that exploits data on a wider sample of schools (not just those near the threshold) in the years before and after their state introduces the CEP (dates are provided in table 1). The staggered introduction of the CEP across states provides within-year variation in exposure to the program, which Ruffini (Forthcoming) and Gordon and Ruffini (2021) leverage to estimate the effect of the CEP on student performance. Figure 3 plots take-up against ISP separately by number of years since introduction for different cohorts. The patterns indicate a lag in take-up among eligible schools. Both average adoption rates and the gradient of adoption and ISP appear more pronounced in the second year and beyond. Interestingly, schools in the second wave of state adoption (New York, Ohio, West Virginia, and Washington, DC) display the strongest kink in participation rates at 40%; these may be states where our NCES

ISP proxy aligns more closely with the true ISP.

Figure 3: School CEP Adoption Share by State Adoption Wave



Notes: Wave 1: 2011-2012 (Pilot year. Illinois, Kentucky, Michigan); Wave 2: 2012-2013 (New York, Ohio, West Virginia, DC); Wave 3: 2013-2014 (Georgia, Florida, Maryland, Massachusetts); Wave 4: 2014-2015 (all other states, CEP adopted nationally).

4.2 Household spending data

To measure the impact of the CEP on household spending, we use data from Nielsen's Homescan, which contains information on all grocery purchases for a panel of American households from 2011-2016. Table 2 reports summary statistics for the 43,945 panelist households, of which 20% contain at least one school-aged child.[12] Crucially, the dataset contains each household's ZIP code. Because we do not know the identity of the school that children within the household attend, we instead measure each household's exposure to the CEP as the average eligibility and participation of schools within the ZIP code where the household resides. The results presented below are robust to measuring eligibility with the average ISP in the household's ZIP code.

[12]Our panel is smaller than the raw panel Nielsen panel, since we aggregate household purchases to the academic year and only keep household-years where we observe positive purchases in all 12 months of the academic year.

Table 2: Household Summary Statistics for AY 2014-2015

	Mean	Std.	Min	Max
Household Spending				
Total Spending ($ hundreds)	46.93	26.68	0.10	567.05
Total Grocery Spending ($ hundreds)	24.31	19.01	0.01	221.84
Total Lunch Meat Spending ($ hundreds)	0.43	0.58	0.00	11.11
Household Characteristics				
Household Size	2.27	1.22	1	9
Household Income ($ thousands)	66.72	43.72	2.5	150
Share of Household with School Age Childeren	0.16	0.37	0	1
Share of White	0.84	0.37	0	1
Share of Black	0.09	0.28	0	1
Share of Asian	0.03	0.18	0	1
Share of Other Race	0.04	0.19	0	1
Count	27,357			

Notes: Spending is calculated on an annual basis from September - August in order to align with the academic calendar.

4.3 Store sales data

The third data source crucial for our analysis is the Nielsen Scantrack data, which contains weekly sales and quantities by product (Universal Product Code or UPC) collected by point-of-sales systems located in over 20,000 participating grocery stores across the US in 2011 and 2016.[13]

Because supermarkets typically stock thousands of products, we construct an inflation index to capture changes in the price of a fixed bundle of goods, whereby avoiding any product heterogeneity biases. Following Beraja et al. (2019), we measure inflation for continuing UPCs: those sold in a given store in every month in the current year and at least one month in the previous year. The calculation proceeds in two steps. First, for each product module j, we calculate a month-on-month arithmetic inflation index for each store s. Let u denote a particular product (UPC), and $U_{j,s,m}$ be the set of products sold in store s in group j in month m. Product-module level inflation for a given month is defined as:

$$\frac{P_{s,j,m}}{P_{s,j,m-1}} = \frac{\sum_{u \in U_{j,s,m}} p_{u,s,m} q_{u,s,y(m)-1}}{\sum_{u \in U_{j,s,m}} p_{u,s,m-1} q_{u,s.y(m)-1}}$$

where $p_{u,s,m}$ is the unit price at which UPC u is sold in store s in month m and $q_{u,s,y(m)-1}$

[13]The Nielsen data is provided by the Kilts-Nielsen Data Center at the University of Chicago Booth School of Business; refer to http://research.ChicagoBooth.edu/nielsen for information on availability and access.

is the quantity of UPC u sold in store s in the calendar year preceding month m. We then aggregate across product modules using a Tornqvist index:

$$\frac{P_{s,m}}{P_{s,m-1}} = \prod_{j \in J} \left(\frac{P_{s,j,m,y}}{P_{s,j,m-1,y}} \right)^{\frac{S_{s,j,m}+S_{s,j,m-1}}{2}}$$

where $S_{s,j,m}$ denotes the expenditure share of product module j in store s in month m. Store-level monthly inflation is therefore a Tornqvist aggregate of Laspeyres-style lagged-weight arithmetic indexes at the product module-store level. To obtain the price level of each store s in month m, $P_{s,m}$, we take a rolling average of store-level monthly inflation from January 2010, during which the index is set to 1.

To study how store prices and revenues respond to CEP participation, we aggregate the data to the annual level, with each year running from September to August to reflect the academic calendar. Sales for the 2014-2015 school year, for example, are total sales from September 2014 to August 2015, while the price index is the average of the monthly price index $P_{s,m}$ over the same period. We calculate revenue and price indexes for breakfast and lunch foods separately, where we hand-code product categories based on Nielsen descriptions. See Appendix table 13 for a list of breakfast and lunch foods groups. In our main analysis, we focus on grocery stores because they account for the lion's share of breakfast and lunch spending.[14]

For each store i, we calculate exposure to the CEP as the average participation and eligibility of the schools that share the same ZIP code. Table 3 contains summary statistics on stores as well as the schools in our sample in the 2014/2015 school year.

[14]We also limit the sample to stores for which we have a breakdown of sales by breakfast and lunch foods.

Table 3: Summary Statistics for the 2014-2015 School Year

	Mean	Std.	Min	Max
Store Characteristics				
Annual Revenue ($ millions)	1.58	0.90	0.14	10.26
Lunch and Breakfast Revenue ($ millions)	0.50	0.28	0.00	3.06
Lunch Meat Revenue ($ millions)	0.02	0.01	0.00	0.18
Price Index: All Goods	1.46	0.20	1.05	2.94
Count	5,575			
Zipcode Characteristics				
Average NCES ISP Proxy (% of enrollment)	42.03	49.37	0.00	100.00
Average I[NCES ISP $>$ 40%]	40.49	39.64	0.00	100.00
Average Participation Rate (%)	12.89	27.75	0.00	100.00
Zip Chain AvgEligible	0.46	0.10	0.05	0.94
Zip Chain PartCEP	1.34	0.70	-0.77	3.56
Zip Chain PartCEP	0.13	0.04	0.00	0.26
Household Annual Income	67.62	26.56	13.85	208.13
Share of African American Population	8.37	14.20	0.00	97.20
Zip Price Index: All Goods	1.47	0.19	1.07	2.99
Average number of Nielsen Stores	1.42	0.82	0.00	6.00
Average number of CBP Stores	5.80	5.03	0.00	61.00
Count	3,279			
Chain-Level Characteristics				
Average NCES ISP Proxy (% of enrollment)	38.94	25.71	0.00	100.00
Average I[NCES ISP $>$ 40%]	40.83	22.63	5.32	94.23
Average Participation Rate (%)	6.07	8.45	0.00	50.87
Count	57			

Notes: Store summary statistics based on a balanced panel of grocery retailer sales in the Nielsen ScanTrack data (2010-2016). ZIP code and chain characteristics from ScanTrack and NCES data.

5 The CEP Demand Shock Evidence from Households

We begin by documenting how the CEP changes demand for groceries. Specifically, we present evidence that the direct beneficiaries of the program (households with school-aged children) reduce their expenditures when their neighboring schools adopt the CEP. Because adoption may be correlated with other time-varying factors that directly affect grocery spending, we use adult-only households as a control group for those households with school-aged children (only the latter are directly affected by the CEP). The identification assumption is that these households are equally affected by any relevant time-varying factors.

Our regression specification is:

$$\ln E_{ht} = \gamma_0 + \gamma_1 \cdot CEP_{s(h),t} + \gamma_2 \cdot CEP_{s(h),t} \times Kid_h + \Gamma_h + \Omega_t \times State_h + \epsilon_{ht} \qquad (2)$$

where E_{ht} is the sum of household h's expenditures in year t; Kid_h is an indicator for whether household h includes a school-aged child; $CEP_{s(h),t}$ is the weighted average CEP adoption of the elementary, middle, and high schools nearest to household h's ZIP centroid at time t; and Γ_h are household fixed effects, which control for time-invariant differences in spending across households. Results are presented in table 4a. In even-numbered columns, we include a triple interaction of $CEP_{s(h),t} \times Kid_h \times \ln(Income_h)$, with an eye towards understanding which households substitute from store-bought lunch to school lunch when the CEP is introduced. In theory, the lowest income households already receive lunch for free, and so adoption of the CEP for this group could operate only through stigma; because of its link to family finances, these eligible students may have been embarrassed to receive free school lunch under the traditional NSLP (Moffitt, 1983). In contrast, for higher income households, the CEP changes the relative price of the two goods.

The baseline results, presented in column 1 of table 4a, suggest that the CEP reduces grocery store expenditures of households with school aged children by around 7% relative to households without children. This difference is both economically and statistically significant (approximately $200 annually for households in our sample). The results do not speak to heterogeneity across income groups: the triple interaction term in column 2 is statistically insignificant and the confidence interval is wide. Columns 3 and 4 shows similar effects for product modules that we categorize as breakfast and/or lunch foods (see appendix table 13 for a list of products). Columns 5 and 6 show that CEP adoption lowers the number of shopping trips undertaken by households with school-aged children by 6-7%.

Table 4b replicates these results focusing only on expenditures at and trips to the Nielsen RMS grocery retailers, which tend to belong to larger chains. Households with school-aged children reduce their grocery expenditures at RMS retailers by 13% relative to households without school-aged children and reduce their trips by over 6%. The outsized effects at Nielsen retailers suggests that the substitution towards free school lunch causes households to substitute towards smaller, independent retailers for their remaining grocery shopping needs.

Table 4: Effect of the CEP on Household Spending

Panel a: Expenditure and Trips to All Stores

	Food Expenditures		B/L Expenditures		Number of Grocery Trips	
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	0.021*	0.025**	0.019	0.023*	0.013	0.018**
	(0.011)	(0.011)	(0.012)	(0.012)	(0.008)	(0.008)
CEP x School Age Kid	-0.066**	-0.070**	-0.072**	-0.075**	-0.067***	-0.072***
	(0.028)	(0.028)	(0.029)	(0.029)	(0.021)	(0.021)
CEP x ln(Income)		0.030**		0.028**		0.030***
		(0.013)		(0.014)		(0.010)
CEP x School Age Kid x ln(Income)		-0.034		-0.036		-0.019
		(0.033)		(0.035)		(0.028)
R-Squared	0.859	0.859	0.851	0.851	0.868	0.868
Observations	249,064	249,064	249,064	249,064	249,064	249,064

Panel b: Expenditure and Trips to RMS Stores

	Food Expenditures		B/L Expenditures		Number of Grocery Trips	
	(1)	(2)	(3)	(4)	(5)	(6)
CEP	0.050**	0.055***	0.047**	0.052**	0.016*	0.022**
	(0.021)	(0.021)	(0.022)	(0.023)	(0.009)	(0.009)
CEP x School Age Kid	-0.134***	-0.136***	-0.160***	-0.163***	-0.067***	-0.072***
	(0.050)	(0.050)	(0.053)	(0.053)	(0.024)	(0.023)
CEP x ln(Income)		0.030		0.030		0.039***
		(0.020)		(0.022)		(0.011)
CEP x School Age Kid x ln(Income)		-0.065		-0.058		-0.050*
		(0.071)		(0.074)		(0.030)
R-Squared	0.844	0.844	0.835	0.835	0.87	0.87
Observations	185,267	185,267	185,267	185,267	185,267	185,267

Notes: Standard errors, clustered at the ZIP level, in parentheses. All specifications include state-by-year and household fixed effects. All outcomes are measured in logarithms. Log income is de-meaned. All specifications include an indicator for whether the household includes a school-aged child.

Figure 4 illustrates this difference-in-differences strategy by showing the difference in spending between households with and without kids each year before and after their local school adopts the CEP.[15] Before adoption, the spending difference between households with school-aged children relative to those without is fairly stable. Consistent with a causal effect, statistically detectable differences emerge only after the CEP is introduced.

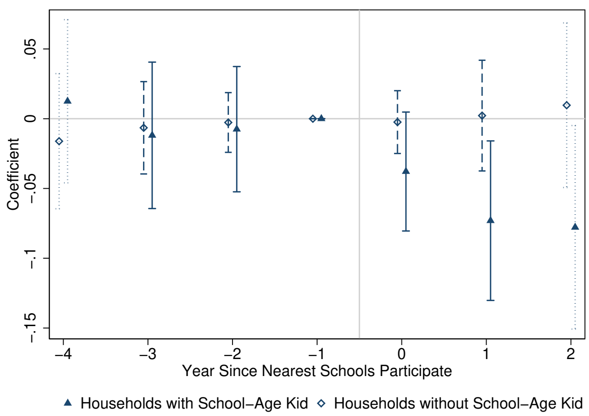
These estimates imply a substantive effect of the CEP on grocery demand. Households with school-aged children comprise approximately 50% of the population, so that grocery stores adjacent to schools that take-up CEP would expect at least a 4% decline in overall sales.[16] In the next section, we explore how grocery stores respond to this shift in their residual demand curves.

[15]For visual simplicity, we code each household's exposure as binary here by considering a households treated if its nearest school adopts the CEP.

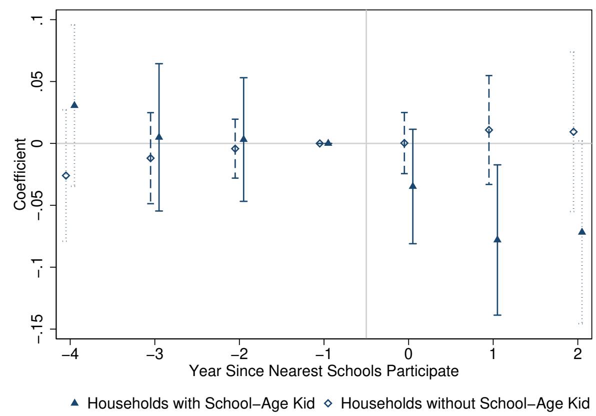
[16]`https://nces.ed.gov/programs/digest/d09/tables/dt09_019.asp`. Assuming that the average household with children spends more on groceries than the typical adult-only household.

Figure 4: Spending Differences between Household with/without a School-Aged Child

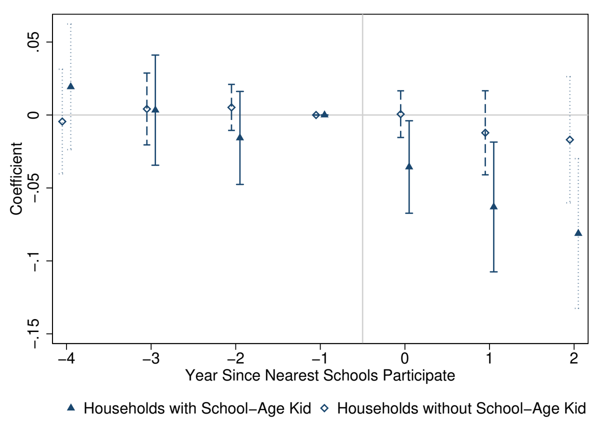
(a) Grocery - Food



(b) Grocery - Breakfast/Lunch



(c) Grocery - Number of Trips



Notes: Using 2007–2017 Homescan data, these figures present the treatment effect of CEP participation on household food and breakfast/lunch expenditures and number of grocery shopping trips. The data includes only the households with the nearest school that has ever participated in the program. All regressions control for state-by-year and household fixed effects. Standard errors are clustered at the ZIP level. The academic years beyond [t-4, t+2] are binned up on both ends to ensure balanced panel of data, where t denotes the year of participation.

6 Retailer Responses to the CEP

We now turn to our main object of interest: the supply-side response to the Community Eligibility Provision, with a focus on whether and to what extent retailers adjust prices. Our results in section 5 suggest that the average household with school-aged children reduces expenditures in RMS grocery stores by as much as 15% when their neighboring school adopts the CEP, extending free lunch and breakfast to all students. Our results are consistent with substitution away from home-made lunch (a positive cross-price elasticity). The conceptual framework that we develop in section 3 highlights that by changing the demand elasticity for groceries, the CEP may cause retailers to increase or decrease prices–whether or not retailers engage in uniform pricing. Because CEP adoption is endogenous to local economic conditions, we exploit the discontinuity in CEP eligibility rules to identify how the program affects store pricing decisions.

6.1 Econometric Model

We model the price index of store i in year t as a function of the CEP participation rates of neighboring schools (in the same ZIP code) in that year. We also include store fixed effects Ω_i and year-by-county fixed effects $\Gamma_t \times \Delta_{county(i)}$ to pick up any time-invariant drivers of store revenue (e.g., store size) and any county-wide shocks to spending.[17] Our specification of interest is therefore:

$$y_{it} = \beta_0 + \beta_1 \cdot CEP_{it} + \Omega_i + \Gamma_t \times \Delta_{county(i)} + \epsilon_{it}. \tag{3}$$

If grocery stores adjust prices when their local school adopts the CEP, then $\beta_1 \neq 0$.

We present estimates of (3) in column 1 of table 5. The coefficient on the CEP adoption rate suggests that neighboring school participation reduces grocery store prices on the order of 0.9%. The drop is less pronounced for the breakfast/lunch category and for lunch meat in particular. We do not interpret these estimates of equation (3) as causal because participation in the CEP is elective. Our concern is that schools might select into the program on the basis of local economic trends (more granular than at the county level), potentially confounding OLS estimates. To identify a causal effect of the CEP, we adopt a difference-in-differences approach.

Table 5: OLS Results of Prices on CEP Adoption

	(1) All	(2) B/L	(3) Lunch Meat
Store Zip CEP	0.010	-0.010**	-0.001
	(0.007)	(0.004)	(0.003)
R-Squared	0.897	0.903	0.931
Observations	53,059	53,059	53,059

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include store and county-year fixed effects. Standard errors are clustered at store level. Price indices are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year.

[17] A second motivation for including county-by-year FE are concerns raised in Sun and Abraham (Forthcoming) and Goodman-Bacon (2021). Including these FE narrows the control group in our analysis to others stores within the same wave.

Our estimation strategy exploits two features of the CEP: the staggered roll-out of the CEP across four waves and the requirement that a school's ISP equal or exceed 40% to qualify for the provision.[18] The first source of identifying variation allows us to construct an estimator that compares changes in store prices in early and late adopting states. This variation is similar to the identification in Hoynes and Schanzenbach (2009), who study the effect of SNAP on food spending. Our chief concern is that early-adopting states may differ systematically from late adopters in ways that affect year-to-year changes in store prices. We therefore construct a difference-in-differences estimator using the 40% ISP eligibility requirement.

In our preferred specification, stores near schools below the 40% threshold act as controls for those near schools above the threshold. This strategy is similar in spirit to a regression discontinuity design, but exploits the entire dataset rather than the set of schools near the threshold.[19] Our identification assumption is that changes in the relationship between ISP and an outcome of interest below/above 40% are driven by the CEP, rather than changes in the underlying characteristics of those stores.

Our preferred specification for an outcome y for store i is:

$$\begin{aligned} y_{it} = {} & \alpha_0 + \alpha_1 \cdot 1\left[StateAdopt\right]_{it} + \alpha_2 \cdot 1\left[StateAdopt\right]_{it} \times shareISP40_i \\ & + \Omega_i + \Gamma_t \times \Delta_{county(i)} + \omega_{it} \end{aligned} \tag{4}$$

where $1[StateAdopt]_{it}$ is an indicator that store i's state had adopted the CEP by year t and $shareISP40_i$ is the share of neighboring schools with an 2010 ISP of 40 or above. The coefficient of interest is α_2, which governs the relationship between prices and the interaction between the ISP threshold and state adoption. The coefficient α_2 therefore captures an intent-to-treat effect of the CEP–the change in price for local stores when their nearest school becomes eligible for the program. Our baseline specification includes store fixed effects, which control for time-invariant determinants of pricing, and county-by-year fixed effects, which control for county-wide shocks to the grocery industry. Importantly, all variants of this specification allow the relationship between ISP and store prices to differ systematically across early and late adopting states for reasons apart from the CEP. The identifying assumption is that any change in this relationship at 40% stems from differences in CEP eligibility.

In our preferred specification, we adapt equation (4) to allow for an important feature of grocery retail in the U.S.. As described in DellaVigna and Gentzkow (2019), among others, most retail chains featured in the Nielsen dataset employ uniform pricing, wherein prices for the same product (UPC) do not vary across broad geographical regions. Uniform pricing

[18] Or group of schools', termed an LEA.

[19] Recall that RDD is not feasible given our proxy for ISP precise measurement of the running variable (Pei and Shen (2017)).

implies that any individual grocery outlet is unlikely to adjust prices in response to a local CEP demand shock. However, a retail chain that is highly exposed to the CEP may adjust prices across a broad swathe of stores. In other words, a chain with a high share of stores that experience the CEP demand shock may respond along the price margin, changing prices even in stores that are not directly exposed themselves.

To test the uniform pricing hypothesis, we construct chain-level analogues of CEP adoption (the average adoption of retail outlets in the chain) and exposure (the average neighboring school CEP eligibility of retail outlets in the chain). In our augmented specification, we examine whether individual retailer and/or chain exposure affects pricing.

6.2 Price Responses

Table 6 presents results of local store-level and chain-wide exposure to CEP adoption on prices. Columns 1 and 3 show a null effect of local store-level exposure on prices (point estimates are less than 1%). In contrast, we find a robust negative effect of chain exposure on price whether or not we control for local exposure (columns 2 and 3). A one-standard deviation increase in chain exposure leads to a 2.3% decline in the price index.

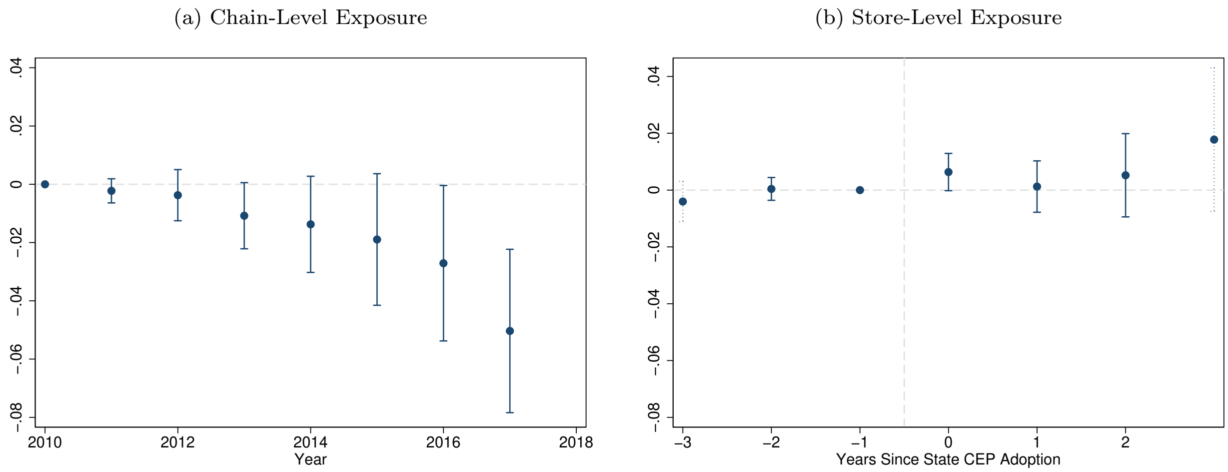
We present the results graphically in figure 5. The left panel shows how prices drop over time for chains with high average eligibility, measured as the share of stores that operate near schools with an ISP$\geq$ 40, relative to chains with low average eligibility. Consistent with the staggered roll-out of the CEP between 2011-2014, prices decline gradually. The right panel shows how prices evolve for stores that neighbor eligible vs ineligible schools before and after the state adopts the program. We see no evidence of price changes in response to local exposure.

Table 6: Effect of CEP Exposure on Prices

	All			B/L		
	(1)	(2)	(3)	(4)	(5)	(6)
State Adopt x Store	0.001		0.007	-0.006*		-0.000
Zip Percent Eligible	(0.005)		(0.005)	(0.003)		(0.003)
Chain Average of		-0.022***	-0.023***		-0.022***	-0.022***
(State Adopt x Store Zip Percent Eligible)		(0.003)	(0.003)		(0.003)	(0.003)
Observations	53,059	53,059	53,059	53,059	53,059	53,059
R-Squared	0.897	0.897	0.897	0.903	0.904	0.904

Notes: Outcome variable is a price index constructed using store-weights. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include store and county-year fixed effects. Standard errors are clustered at store level. The price index is constructed at the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. Chain exposure is normalized.

Figure 5: Effect of CEP Eligibility Exposure on Prices



Notes: Plots regression coefficients from a specification that includes store and county × year FE. In addition, plot 5a controls for local eligibility to the CEP and plot 5b controls for chain exposure. Local CEP eligibility is an enrollment-weighted average of the eligibility of the elementary, middle and high school nearest to the store's ZIP code centroid. Chain exposure is measured as the average of the enrollment-weight average eligibility of schools that neighbor stores in the chain (we normalize this measure for ease of interpretation).

These results offer two implications: first, they suggest that the spillovers of the CEP are substantial, lowering prices for households with and without children. If redistribution to consumers in areas with high-exposure chains but low rates of local adoption is desirable, then these spillovers present an argument in favor of in-kind giving. Second, they provide evidence for how market structure–in particular, the configuration of retail chains–affects the propagation of demand shocks in the economy. Relative to other work studying the effects of demand shocks on prices (e.g., Leung and Seo, 2018 and Stroebel and Vavra, 2019), our results confirm that effects can be large, but also suggest that these effects depend crucially on the spatial distribution of chains.

6.3 Impact of the CEP on Store Revenue

We next document how store and chain CEP exposure affects retailer revenues. Examining revenues provides a check on our econometric model in two ways. First, we confirm that local rather than chain CEP exposure reduces store revenues. While uniform pricing implies that CEP price effects ought to propagate across chains, the first order effect of the CEP on revenue ought to come from a reduction in grocery demand in stores near households with children who attend schools that adopt the CEP. Second, we reconcile estimated CEP retailer revenue effects with the estimated household expenditure effects described in section 5, providing reassurance that the difference-in-difference identifies the causal effect of the

CEP.

We report our baseline estimates of specification 4 in table 7 and the graphical analogue in figure 6. The coefficient in column 1 implies that stores near schools that are all eligible for the CEP earn 2.9% lower revenues than those neighboring ineligible schools. Estimates are larger in magnitude if we focus on the sales of lunch meats (column 3). We expect that CEP adoption disproportionately affects breakfast and lunch food sales because these categories should suffer most acutely if families no longer pack lunch for children under the CEP. Figure 6 shows that this difference in revenues emerges gradually after state adoption of the policy, consistent with the slow take-up of the CEP among eligible schools shown in figure 3. Appendix B.2 shows that effects are concentrated among retail outlets located in areas with a higher share of school-aged children. Appendix B.3 shows that the difference between treatment and control stores is robust to the inclusion of state-by-year FE and eligibility-by-year FEs. As an additional check, Appendix B.4 shows that revenues fall more in categories with high family appeal. In Appendix B.5, we estimate the effect of the CEP separately by wave of adoption to check for heterogeneous treatment effects.

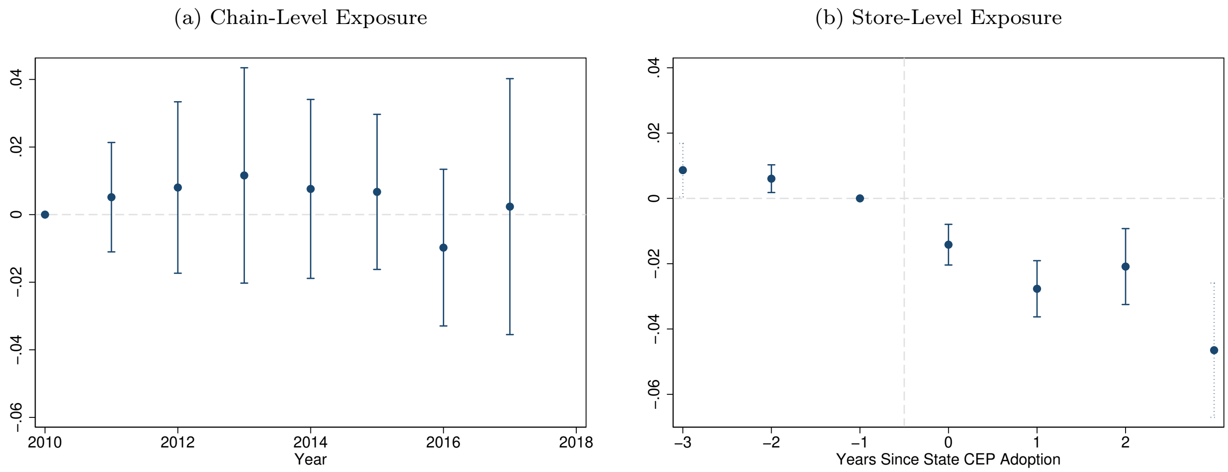
The results in table 7 further indicate that chain CEP eligibility has no detectable effect on overall store revenue. That is, stores that are not directly exposed to the CEP (their local school is ineligible) do not see a revenue decline overall, even if other retail outlets in the same parent chain are exposed. Taken together, these findings reassure us that chain exposure is not correlated with omitted determinants of grocery demand (conditional on the set of fixed effects in specification 4).

Table 7: Effect of CEP Exposure on Log Grocery Revenue

	All			B/L		
	(1)	(2)	(3)	(4)	(5)	(6)
State Adopt x Store	-0.028***		-0.028***	-0.031***		-0.030***
Zip Percent Eligible	(0.005)		(0.005)	(0.005)		(0.005)
Chain Average of		-0.002	0.001		-0.005	-0.002
(State Adopt x Store Zip Percent Eligible)		(0.003)	(0.003)		(0.003)	(0.003)
Observations	53,059	53,059	53,059	53,059	53,059	53,059
R-Squared	0.986	0.986	0.986	0.985	0.985	0.985

Notes: All regressions include store and county-year fixed effects. Standard errors are clustered at store level and reported in parentheses (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. Chain exposure is normalized.

Figure 6: Effect of CEP Eligibility Exposure on Log Grocery Revenues



Notes: Plots regression coefficients from specification that includes store and county $\times$ year FE. In addition, plot 6a controls for local eligibility to the CEP and plot 6b controls for chain exposure. Local CEP eligibility is an enrollment-weighted average of the eligibility of the elementary, middle and high school nearest to the store's ZIP code centroid. Chain exposure is measured as the average of the enrollment-weight average eligibility of schools that neighbor stores in the chain (we normalize this measure for ease of interpretation). The dashed line indicates periods with data for a subset of waves.

6.4 Extensive Margin Responses

The reduction in residual demand documented above suggests that the grocery outlets hardest hit by the CEP may exit, and that the CEP may deter entry of new retailers. As a less dramatic form of exit, retailers may eliminate and/or introduce different products in response to the CEP. In this subsection, we directly estimate the effect of the CEP on retailer churn and product assortments.

We adopt specification 4 so that z denotes ZIP code (rather than an individual retail outlet) and the outcome of interest is the number of firms. Note that we include ZIP code and county-by-year fixed effects (but not retailer FE), as in the following equation

$$\begin{aligned} NFirms_{zt} = \alpha_0 + \alpha_1 \cdot 1\left[StateAdopt\right]_{zt} + \alpha_2 \cdot 1\left[StateAdopt\right]_{zt} \times shareISP40_z \\ + \Omega_z + \Gamma_t \times \Delta_{county(z)} + \epsilon_{zy}. \end{aligned} \tag{5}$$

Table 8 presents estimates of how the CEP affects exit, entries, and the total number of firms for the RMS sample. Note that we exclude instances when an entire chain enters or exits the RMS dataset; our concern is that these entries and exits are a function of the RMS dataset itself, and do not reflect true changes in the local retail environment. We also exclude stores that we cannot match to a school with CEP participation and eligibility data based on the retailer's ZIP code. Unfortunately, the CEP participation data is spotty, so this eliminates about 20% of the RMS stores. Appendix table 14 displays the sample creation criteria.

The results, shown in table 8 column 1, are inconclusive: high-eligibility ZIP codes support slightly fewer Nielsen stores (approximately 1%) following the adoption of the CEP, but the difference is not statistically significant. The signs of the point estimates indicate a reduction in entries (column 4) and an increase in attrition among existing stores (column 3), but they are also economically small and statistically insignificant. Overall, churn is quite limited in our sample: only 5% of ZIP codes experience any entry or exit among Nielsen retailers between 2012-2016. This evidence suggests that selection bias is not a concern in our price and revenue regressions (tables 6 and 7) because these results consider effects on existing stores. Because entry and exit decisions might operate over a longer time horizon than we study, future work might revisit this margin of retailer response.

Table 8: CEP and Churn among Nielsen Firms

	(1) N Firms	(2) Change	(3) Exits	(4) Entries
State Adopt x Percent Eligible	-0.013	-0.004	0.002	-0.002
	(0.015)	(0.009)	(0.008)	(0.005)
Observations	23,262	23,262	23,262	23,262
Zip Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.954	0.421	0.42	0.424
Mean of Dependent Var	1.445	-.019	.035	.016

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include ZIP and county-year fixed effects, are clustered at the ZIP level, and include a constant. The "Percent Eligible" Method assigns each ZIP code a value between 0 and 1 that represents the percent of stores in that ZIP code whose nearest school's ISP is greater than 40%. This is an average of binary values.

Motivated by findings in the SNAP literature (e.g., Jaravel, 2018), we next consider how retailers adjust assortments in response to the CEP. We employ two measures of product assortments: a count of UPCs and a variety index that weights UPCs by their share of national category sales. The index is meant to capture differences in the appeal and importance of different UPCs following Feenstra (1994).[20] Results are presented in table 9 and indicate a modest decline in both measures of assortment following the state adoption of the CEP. In contrast to the price and revenue effects we document above, assortments appear to adjust to both local and chain exposure to the CEP. This suggests that there is more local discretion over assortments than prices, but that chain identity still matters, *e.g.*, because contracts with distributors are negotiated at the chain level. While the signs of these estimates are interesting, from a welfare perspective, the magnitudes reported in table

[20]Specifically, the variety index calculates the proportion of products (UPCs) sold by store s in month m relative to the total set of products carried across all stores. Each product is weighted by its national sales in month m. The final index is calculated as $V_{s,m} = \sum_{u \in U_{s,m}} \left(\frac{v_{u,m}}{\sum_{u \in U_m} v_{u,m}} \right)$ where $v_{u,m}$ denotes the national sales of product u in month m.

9 are small under typical estimates of substitution elasticities in the literature. For example, a back-of-the-envelope calculation borrowing from Kroft et al. (2021) suggests that a 0.4% percent reduction in variety reduces welfare by 0.05%.

Table 9: CEP and Product Assortments

Number of UPCs			
	(1) All	(2) B/L	(3) Lunch Meat
State Adopt x Store Zip Percent Eligible	-0.005***	-0.005***	-0.016***
	(0.002)	(0.002)	(0.003)
Chain Average of (State Adopt x Store Zip Percent Eligible)	-0.013***	-0.017***	-0.027***
	(0.002)	(0.002)	(0.003)
Observations	53,059	53,059	53,059
R-Squared	0.994	0.994	0.976
Variety Index			
	(1) All	(2) B/L	(3) Lunch Meat
State Adopt x Store Zip Percent Eligible	-0.004***	-0.004***	-0.013***
	(0.001)	(0.002)	(0.005)
Chain Average of (State Adopt x Store Zip Percent Eligible)	0.002	0.002	0.007
	(0.001)	(0.002)	(0.005)
Observations	53,059	53,059	53,059
R-Squared	0.998	0.994	0.981

Notes: Outcome variable in the top panel is the log count of UPCs and in the bottom panel is the variety index described above constructed using store-weights. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Chain exposure is normalized. All regressions include a constant. Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year.

6.5 Accounting for Takeup

Because most eligible schools do not adopt the CEP, we estimate how participation affects stores using instrumental variables to back out the effect of treatment on the treated. To do this, we estimate equation (3) using $1\left[StateAdopt\right]_{it} \times shareISP40_{it}$ as an instrument for the share of local schools that participate in the CEP.[21] This formulation scales the estimator from (4) by take-up in a fashion similar to Hoynes and Schanzenbach (2009). The first stage specification mirrors equation (4), where the dependent variable is the share of schools participating under the CEP (which takes a value between 0 and 1). The first stage results are presented in table 10. They suggest that schools with an ISP above 40% are around 20 percentage points more likely to adopt the provision than schools in adopting states with ISPs below 40%. Recall that some schools with low ISPs may qualify for the CEP as part of a larger conglomerate of schools, where the combined ISP is above 40%. Despite this possibility, the results indicate large differences in take-up above and below the

[21]Schools that share the same ZIP code as the store.

ISP threshold.

Table 10: First Stage Results

	(1)	(2)	(3)	(4)
State Adopt x Percent Eligible	0.204***	0.201***	0.193***	0.192***
	(0.008)	(0.008)	(0.008)	(0.009)
Constant	0.010***	0.007***	0.006***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)
ISP Year	2009	2010	2011	2012
R-squared	0.778	0.778	0.776	0.782
F-stat	52,255	53,059	53,021	48,963

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include store and county-year fixed effects. Standard errors are clustered at store level.

Underreporting of CEP participation poses a challenge for this IV strategy. Stephens Jr. and Unayama (2019) highlight that underreporting tends to overstate the IV effects, even if underreporting occurs at random (intuitively, the first stage estimate will be too small while the reduced form estimates are unaffected). They show that the IV estimator converges to $\frac{\beta_1}{p}$, where p is fraction of schools correctly reporting their CEP status. We estimate $\hat{p} \approx 0.7$ in our setting by comparing the school-level NCES participation rates to aggregate participation rates reported by the Center for Budget and Policy Priorities. To adjust for underreporting, we therefore deflate the IV estimates in table 11 by scaling by $\hat{p}$. The adjusted estimates imply that local CEP adoption reduces grocery revenue for the full calendar year by 9.8% (column 1), and a one-standard deviation increase in chain exposure reduces prices by 2.45%.

Table 11: Effect of CEP Participation on Grocery Retailers

	Revenue			Price		
	(1)	(2)	(3)	(4)	(5)	(6)
Store Zip CEP	-0.140***		-0.139***	0.007		0.039
	(0.023)		(0.024)	(0.027)		(0.027)
Chain CEP		-0.003	0.001		-0.033***	-0.034***
		(0.004)	(0.005)		(0.004)	(0.004)
First Stage F-Stat	663	1766	905	663	1766	905
Observations	53,059	53,059	53,059	53,059	53,059	53,059

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. All regressions include a control for the years in which the state has adopted the program, and store and county-year fixed effects. Standard errors are clustered at store level. Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year. Chain CEP is standardized. Instrument is the interaction of dummies for state adoption and the nearest school having 40 percent or more eligible for free and reduced lunch in 2010. The first stage is presented in table 10.

As a benchmark, we compare these estimates to a back-of-the-envelope calculation for the effect of free lunch and breakfast on an average monthly grocery budget, assuming the combined daily cost of lunch and breakfast totaled \$4.15 before the CEP.[22] Then the CEP amounts to a monthly transfer of approximately \$82.93 per child, which we use in conjunction with USDA estimates for the cost of food at home to calculate household CEP savings:[23]

$$
\begin{aligned}
\%\Delta\text{Spending} &= \frac{-\#\text{Children } \times \text{Value of Breakfast \& Lunch}}{\text{Monthly Grocery Expenditures for Family of Four}} \\
&= \frac{-2 \times 82.93}{642.10} \times 100 \\
&= -25.83\%
\end{aligned}
$$

If all Americans were on the "thrifty plan" for the cost of food at home and crowd-out were one-for-one, then we would expect at 25.83% decline in grocery spending for affected families. The reduction in demand is smaller for the "low-cost plan" (19.60%). If participation in school meal programs doubles from 40% to 80% under the CEP, then store revenue would fall 8%.[24] Our estimates are slightly larger, potentially for three reasons: first, lunch spending might constitute a disproportionate share of the grocery bill (if, for example, families eat dinner at restaurants or other on-premise locations); second, fertility rates are higher among low-income women, so that the household expenditure share of children's food is higher (Monte and Ellis, 2014); and finally, families may substitute to other formats (e.g., dollar stores) and smaller, independent supermarkets for their remaining grocery needs, which is consistent with the household shopping patterns that we present in section 5.

7 Welfare Effects of the CEP on Households without School-Aged Children

In this section, we demonstrate how the chain-level price responses estimated in section 6 propagate spatially, distributing benefits beyond ZIP codes with high local take-up. We estimate a store choice model to measure how local grocery costs adjust in response to CEP adoption from the perspective of households without children–households for whom school lunch provides no direct benefit. We estimate the parameters that govern store choice for these households and hold these parameters fixed to measure the indirect benefit of the CEP.

7.1 Model Set-up

A representative household residing in ZIP o allocates their grocery expenditure w across

[22] Based on the average price of school meals from the School Nutrition Association's School Meal Trends & Stats.

[23] Official USDA Food Plans: Cost of Food at Home at Four Levels, U.S. Average, June 2018.

[24] Assume 40% of households contain no children, but only two adults. Then $\%\Delta\text{Revenue} = \frac{0.6 \cdot 0.4 \cdot (-165.86)}{0.4 \cdot 384.5 + 0.6 \cdot 0.6 \cdot (642.1) + 0.6 \cdot 0.4 \cdot (476.24)}$.

the set of stores $s \in S_t$ operating at time t to maximize the following CES utility function:

$$U_{ot} = \left[\sum_{s \in S_t} \psi_{ost}^{\frac{1}{\sigma}} \cdot q_{st}^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}$$

subject to

$$\sum_{s \in S} p_{st} \cdot q_{st} \leq w$$

where q_{st} and p_{st} are consumption and price indexes of store s at time t; $\psi_{ost} > 0$ is the household's perception of the quality of store s at time t; and $\sigma > 1$ is the constant elasticity of substitution across stores.

Optimization implies that the representative household allocates a share s_{ost} of their expenditure to store s as in the following expression:

$$s_{ost} = \frac{\psi_{ost} \cdot p_{st}^{1-\sigma}}{\sum_{s' \in S_t} \psi_{os't} \cdot p_{s't}^{1-\sigma}}. \tag{6}$$

Substituting this share into utility, we have the standard CES result that indirect grocery utility is equal to retail expenditure (w) divided by a retail price index that summarizes the quality and prices of the stores available to households in origin ZIP o at time t:

$$P_{ot} = \left(\sum_{s \in S_t} \psi_{ost} \cdot p_{st}^{1-\sigma} \right)^{\frac{1}{1-\sigma}}.$$

The higher this price index, the more households need to spend to achieve the same grocery utility. The change in grocery utility afforded by a fixed level of expenditure w between two time periods t_0 and t_1 is equal to the inverse ratio of this price index between the two periods, $(P_{ot_1}/P_{ot_0})^{-1}$.

We parametrize perceived quality as follows:

$$\ln \psi_{ost} = \ln \xi_{os} + \ln \tilde{\psi}_{ost} \tag{7}$$

where ξ_{os} is the unobserved time-invariant component of utility from origin o at store s and $\tilde{\psi}_{ost}$ is the unobserved time-varying component of utility from origin o at store s at time t, which we assume to be orthogonal to the program. We further parameterize ξ_{os} as being log-linear in the distance between ZIP o and store s such that $\xi_{os} = \tilde{\xi}_{os} - \tau \cdot \ln d_{os}$. Abstracting from the unobservable components of demand, we can re-write the utility index as follows:

$$\ln\left(\frac{P_{ot_1}}{P_{ot_o}}\right) = \left(\frac{1}{1-\sigma}\right)\left[\ln\left(\sum_{s\in S_{t_1}} d^{\tau}_{od(s)} \cdot p^{1-\sigma}_{st_1}\right) - \ln\left(\sum_{s\in S_{t_0}} d^{\tau}_{od(s)} \cdot p^{1-\sigma}_{st_0}\right)\right] \tag{8}$$

This model adds to a growing literature on store choice, including work by Thomasden (2005), Davis (2006), Houde (2012), and Miller and Osborne (2014). Recent work by Ellickson, Grieco, and Khvastunov (2020) employs a model of store choice to explore the effects of grocery mergers. Their model excludes price as a determinant of grocery demand (we note that price is crucial to our objective of measuring the welfare benefits of the CEP price cut), and instead focuses on consumer heterogeneity. Eizenberg, Lach, and Oren-Yiftach (2021) document high travel costs in Jerusalem that sustain price differences across neighborhoods. As in this paper, their model quantifies how shoppers trade off price and distance, although it builds a logit rather than CES demand specification.

7.2 Estimation

Equation (8) shows that grocery costs are impacted by two factors: (i) changes in the prices charged by continuing stores Δp_{st} and (ii) changes in the set of stores open ΔS_t. The analysis in section 6.4 shows that the CEP had only a small effect on store entry and exit by 2016, so we abstract from changes in the store set and focus on price changes.[25] We compare realized prices charged by stores operating in 2010 to counterfactual price indexes based on the estimated impact of the CEP.

Specifically, we calculate:

$$\ln\left(\frac{P_{o\hat{t}_1}}{P_{ot_o}}\right) = \left(\frac{1}{1-\sigma}\right)\left[\ln\left(\sum_{s\in S_{t_0}} d^{\tau}_{od(s)} \cdot \left(p_{st_0} + \widehat{\Delta p_s}\right)^{1-\sigma}\right) - \ln\left(\sum_{s\in S_{t_0}} d^{\tau}_{od(s)} \cdot p^{1-\sigma}_{st_0}\right)\right] \tag{9}$$

where $\widehat{\Delta p_s}$ is the estimated impact of the CEP on the price level charged by store s as a function of its ZIP code $d(s)$ and chain $c(s)$. We predict this impact using the price results in column 3 of Table 11; that is, $\widehat{\Delta p_s} = \hat{\beta}_1 \cdot CEP_{d(s)2016} + \hat{\beta}_2 \cdot ChainCEP_{c(s)2016} + \hat{\beta}_3 \cdot CEP_{d(s)2016} \cdot ChainCEP_{c(s)2016}$.

In the model, the impact of a price adjustment at a store s on grocery costs in an origin ZIP code o depends on the distance of that ZIP code from the store ZIP code, $d_{od(s)}$, mediated by the distance elasticity τ and the elasticity of substitution between stores σ. We proxy for d_{od} with the distance between the centroid of ZIP code o to the centroid of ZIP code d when $o \neq d$ and the mean distance from two points in a circle with the same area as ZIP code o

[25] The model can be adjusted to account for extensive margin responses to the CEP, both at the store-level and the product-level within stores.

when $o = d$. We use 2010, the year before the CEP's inception, as the initial period t_0, and 2016, the last year that we have price data for, as our final year t_1.

We estimate the key demand parameters σ and τ using maximum likelihood. Interpreting the right-hand side of equation (6) as a probability and denoting the number of individuals residing in origin ZIP code o and shopping at store s in academic year t as ℓ_{ont}, the log likelihood function is

$$\ln \mathcal{L} = \sum_{ost} \ell_{ost} \ln \left[\frac{\psi_{ost} \cdot p_{st}^{1-\sigma}}{\sum_{s' \in S_t} \psi_{os't} \cdot p_{s't}^{1-\sigma}} \right] = \sum_{ost} \ell_{ost} \ln \left[\frac{d_{od(s)}^{\tau} \cdot \tilde{\psi}_{ost} \cdot p_{st}^{1-\sigma}}{\sum_{s' \in S_t} d_{od(s')}^{\tau} \cdot \tilde{\psi}_{os't} \cdot p_{s't}^{1-\sigma}} \right] \tag{10}$$

where, in the second equality, perceived destination quality is parameterized as in equation (7).

Estimation proceeds using a Poisson pseudo-maximum-likelihood estimator (PPMLE) where price indexes are calculated from annual store-level data and expenditures are taken from HMS data on the purchases that adult-only households residing in each ZIP code make in each destination store. This estimator relies on the assumption that time-varying origin-store match quality is mean-independent from distance and store prices, conditional on controls ξ_{ost}; that is, $\mathbb{E}\left(\tilde{\psi}_{ost} | d_{od(s)}, p_{st}, \xi_{ost}\right) = 1$ (Silva and Tenreyro, 2006). In our baseline specification, ξ_{ost} includes origin ZIP-by-year, destination store chain, and destination store ZIP fixed effects.

To address attenuation and endogeneity biases that might impact our σ estimate, we instrument for store prices with the sales-weighted average of the log price level charged by stores in the same chain as the destination store, but located in different DMAs ($\widetilde{ChainCEP}_{d(s)c(s)t}$); the sales-weighted average of the log price level charged by stores in the same chain as those in the origin ZIP code, but located in different DMAs ($\widetilde{ChainCEP}_{d(s_o)c(s_o)t}$); and the interaction of these two instruments ($\widetilde{ChainCEP}_{d(s)c(s)t} \cdot \widetilde{ChainCEP}_{d(s_o)c(s_o)t}$). We follow the control function approach proposed by Lin and Wooldridge (2019), bootstrapping the procedure to obtain consistent standard errors.

This specification yields our preferred estimate for the elasticity of substitution (σ) of 2.8 and distance elasticity (τ) of -2.4. Because stores enter endogenously in response to nearby demand, we test the robustness of these estimates to additional controls for the unobserved quality component of the error term ($\tilde{\psi}_{ost}$). The distance elasticity is robust to these controls. The substitution elasticity increases to 3.5 when the store chain fixed effects are dropped, but this difference is not statistically significant and our results below are almost entirely invariant to this adjustment given the spatial correlation in store-level price changes.

Table 12: Store Choice Elasticities

	(1)	(2)	(3)	(4)
Log Price	0.188			-1.784***
	(0.161)			(0.470)
Log Distance	-2.408***	-2.408***	-0.000	-2.415***
	(0.023)	(0.023)	(0.000)	(0.047)
Log Store Chain Price		-1.769***	1.064***	
		(0.436)	(0.016)	
Model	OLS	RF	FS	2-Step IV
Origin Zip-Store Zip Standard Error Clustering	Yes	Yes	Yes	Yes
F-Statistic			2356	
Pseudo R-Squared	0.724	0.724	0.763	0.724
Observations	392,322	392,322	392,322	392,322

Notes: Standard errors, clustered by ZIP pair, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include a constant. Includes ZIP*year, destination chain, and destination ZIP fixed effects.

7.3 Welfare Results

The welfare impact of the CEP in each origin ZIP code is recovered by plugging the estimates for the impact of the policy on store prices ($\widehat{\Delta p_s}$) and elasticities ($\hat{\sigma}$ and $\hat{\tau}$) obtained above into equation (9). Welfare is measured as the change in grocery costs, inclusive of travel costs and product prices. The population-weighted distribution of these welfare estimates is presented in Figure 7. The median effect is a decrease in grocery costs of approximately 4.5 percentage points. This reduction amounts to an \$9 monthly benefit, given the average monthly grocery expenditure from table 2, which is approximately 10% of the \$80 direct monthly benefit to each child receiving free school meals.[26]

There is a significant degree of variation across ZIP codes. Part of this variation can be explained by the spatial distribution of CEP take-up and the spatial distribution of chains with differential exposure to areas where the CEP is adopted.

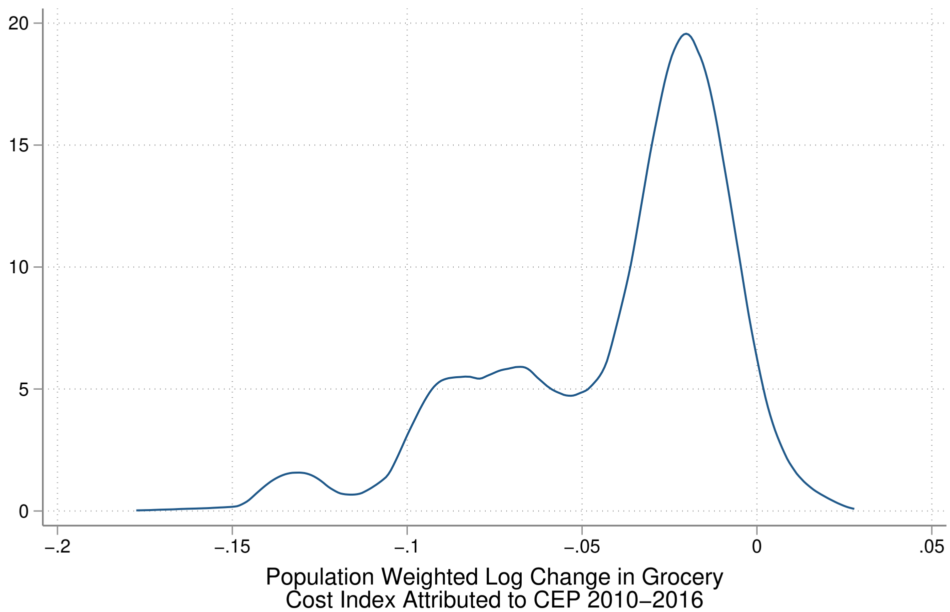
Figure 8 illustrates this variation using the Chicago area as a case study. Panel a shows the share of stores in each ZIP code whose closest school participated in the CEP program in 2016. Panel b shows the average CEP exposure of the chains that operate stores in the ZIP code. Direct exposure is spatially concentrated, but the indirect impact of the program extends beyond these communities to other ZIP code, particularly along the Indiana-Illinois border.

Figure 9 shows the correlation between the indirect benefits of the program and proximity to ZIP codes with different levels of direct and indirect exposure to the CEP program,

[26]Based on the average price of school lunch and breakfast from the School Nutrition Association's School Meal Trends & Stats.

measured as the weighted average of direct and indirect exposure using the distance weights (d^{τ}_{od}). ZIP codes with higher indirect CEP exposure see much larger reductions in grocery costs. In fact, most of the cross-ZIP variation in the estimated welfare impact of the program are explained by variation in the indirect exposure to the program via which chains operate in the vicinity of different ZIP codes. ZIP codes with high densities of heavily exposed chains, which are predicted to reduce their prices in response to the program, saw grocery costs fall by up to 10 percentage points, while ZIPs with low-exposure chains see grocery costs fall by only 3 percentage points. Figure 8 shows that the largest welfare benefits are in ZIPs with zero direct effects and strong indirect effects.

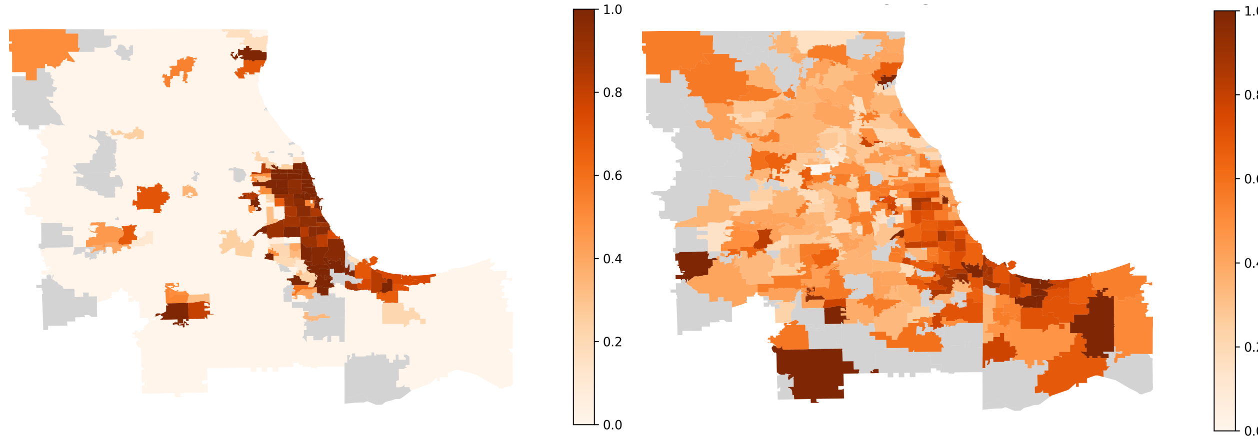
Figure 7: Effects of the CEP on Grocery Costs between 2000 and 2016



Notes: This figure plots the distribution of CEP effects across ZIP codes. For each ZIP code, we calculate the log change in its grocery cost index caused by the CEP. The magnitude of the effect depends on the exposure of the retail chains located in the ZIP code.

Figure 8: Direct and Indirect Exposure to CEP Program in 2016, Chicago

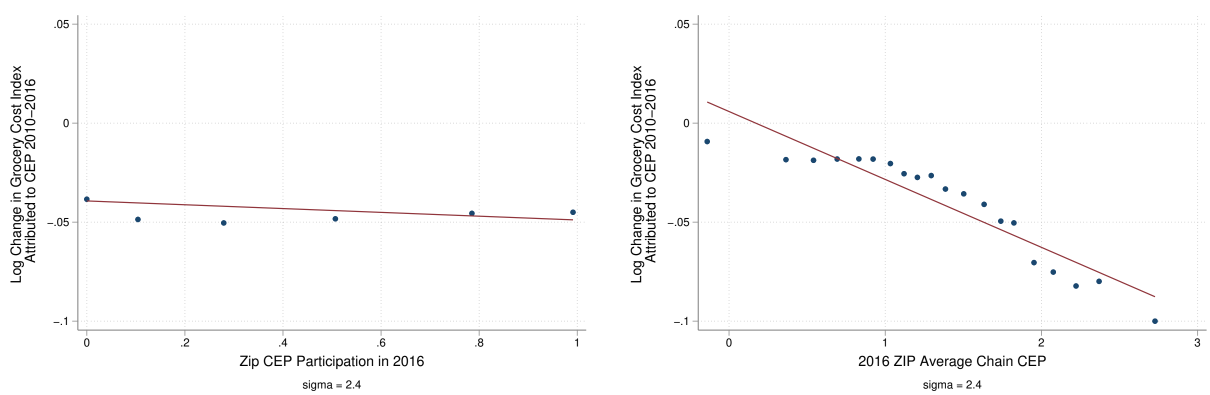
(a) Direct Exposure (b) Indirect Exposure



Notes: Panel a plots direct exposure to the CEP in each ZIP code in the Chicago area in 2016. Direct exposure in a ZIP code is the enrollment-weighted average eligibility of the elementary, middle, and high school nearest the ZIP code centroid. Panel b plots indirect exposure to the program, which is measured as the average CEP exposure of retail grocery chains that operate in the focal ZIP code, *excluding* the exposure in the focal ZIP code.

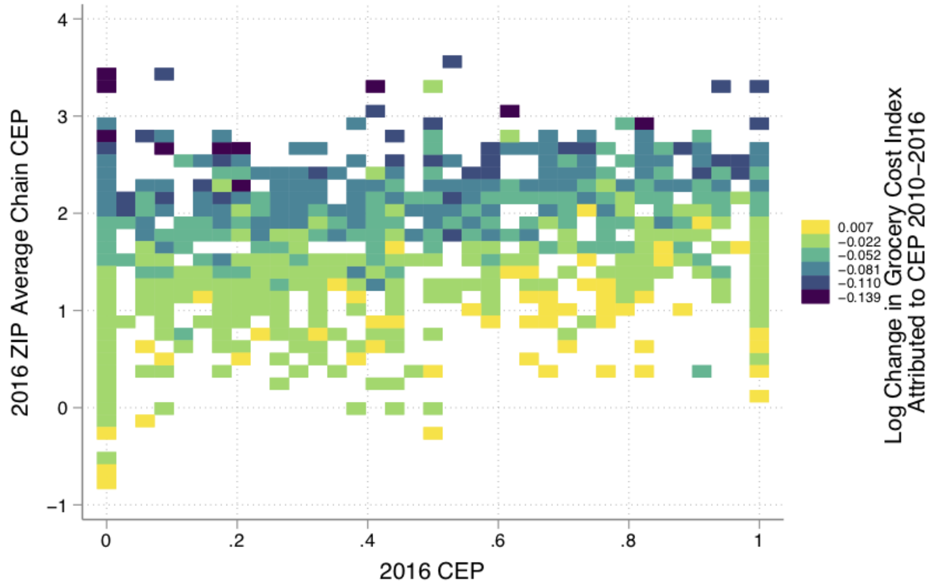
Figure 9: The Effect of Direct and Indirect Exposure to the CEP on Grocery Costs

(a) Direct Exposure (b) Indirect Exposure



Notes: The binned scatterplot in panel a shows the relationship between direct exposure to the CEP and the log change in grocery costs attributable to the CEP. The binned scatterplot in panel b shows the relationship of CEP benefits with indirect exposure. Grocery cost changes are calculated based on our estimates of σ =2.8 and $\tau = -2.4$ from Table 12.

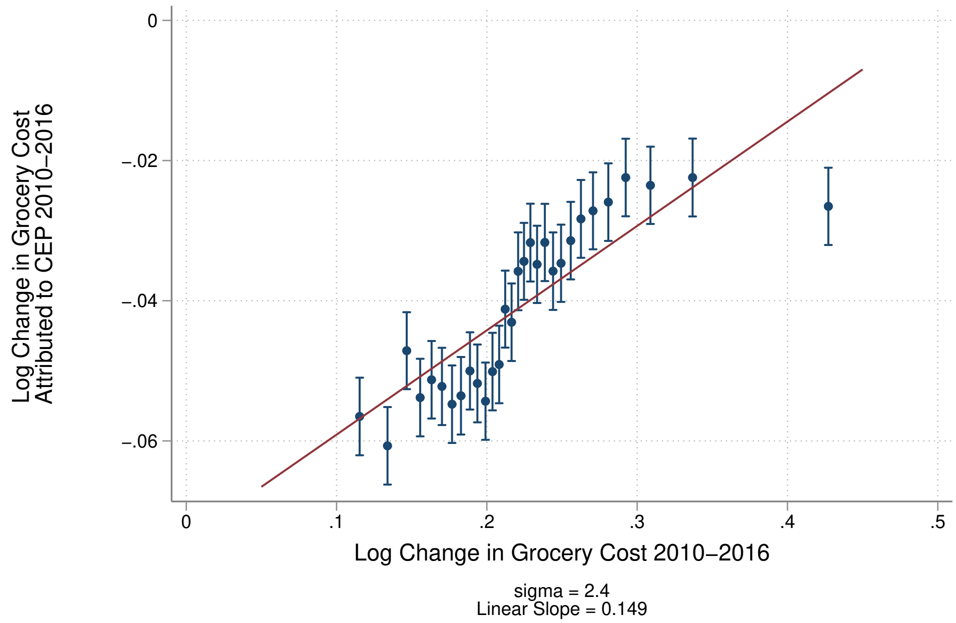
Figure 10: Heat Map of Effect of the CEP on Grocery Costs between 2000 and 2016



Notes: This figure plots the relationship between local CEP participation in 2016, chain exposure to the CEP in 2016, and the change in grocery costs attributable to the CEP. Grocery costs decline mostly steeply in ZIP codes with high chain exposure.

The model above allows us to also calculate the change in ZIP-level shopping costs that result from all changes in store prices (rather than just the changes attributable to the CEP). Figure 11 is a binscatter showing the association between the change in the grocery cost index attributable to the CEP and the total change in the grocery cost index. The x-axis shows that overall grocery price inflation in continuing stores between 2010 and 2016 has resulted in grocery costs increasing in almost all ZIP codes, with most ZIP codes seeing 10 to 30 percent increases in grocery costs. Our analysis suggests that the CEP program counteracted these general trends, mitigating price increases observed in the data over the same period. Figure 11 shows that the CEP program helps to explain some of the spatial variation in price declines: ZIPs with larger predicted spillovers from CEP to shopping costs tended to see smaller increases in their aggregate grocery cost growth.

Figure 11: Welfare Effects of the CEP Relative to Overall Changes in Shopping Costs



Notes: This plot shows the relationship between observed changes in grocery costs across ZIP code from 2010-2016 and the estimated changes in grocery costs attributed to the CEP. The CEP has counteracted a general trend in rising grocery costs.

8 Conclusion

This paper demonstrates that the National School Lunch Program delivers a substantial indirect benefit to communities through its supply-side effects. To establish causality, we leverage an expansion of the NSLP under the Community Eligibility Provision, which requires that participating schools provide free lunch to all students–in essence, lowering the price of a substitue for grocery store lunch products. Panel data on household grocery purchases reveals that households with children reduce their spending by 7% when a local school adopts the CEP. We then show that grocery stores respond to this demand shock by reducing prices.

To quantify the benefit of the program to adult-only households, we estimate a CES model of grocery demand. The welfare estimates suggest that the CEP reduces shopping costs in the median effected ZIP code on the order of 4.5%. For comparison, a back-of-the-envelope calculation suggests that the direct benefit of the NSLP for a household with children amounts to a 25% reduction in shopping costs.

A final finding is that the spatial distribution of retail grocery chains determines the distribution of the indirect benefits. Chain geography is important because many retail grocery chains in the US employ uniform pricing. Accordingly, we find that retailers do not adjust prices in response to local CEP adoption–rather, retail chains adjust prices in response to their overall exposure across outlets. Consequently, some consumers enjoy lower prices even when their local school does not adopt the program. Taken together, our findings show that supply-side forces can meaningfully amplify the benefits of food security policy.

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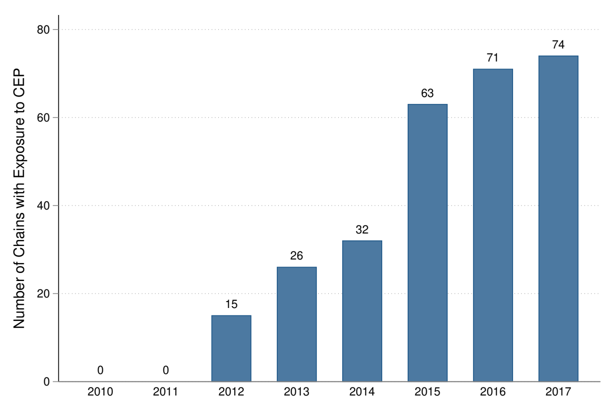
A Data Appendix: Tables & Figures

Table 13: Breakfast and Lunch Food Categories

BAKED GOODS-FROZEN	EGGS
BAKING MIXES	FRESH PRODUCE
BAKING SUPPLIES	FRUIT - DRIED
BREAD AND BAKED GOODS	JAMS, JELLIES, SPREADS
BREAKFAST FOOD	JUICE, DRINKS - CANNED, BOTTLED
BREAKFAST FOODS-FROZEN	MILK
BUTTER AND MARGARINE	NUTS
CEREAL	PACKAGED MEATS-DELI
CHEESE	PREPARED FOOD-READY-TO-SERVE
CONDIMENTS, GRAVIES, AND SAUCES	SALAD DRESSINGS, MAYO, TOPPINGS
COOKIES	SNACKS
COT CHEESE, SOUR CREAM, TOPPINGS	SNACKS, SPREADS, DIPS-DAIRY
CRACKERS	SOFT DRINKS-NON-CARBONATED
DESSERTS, GELATINS, SYRUP	TABLE SYRUPS, MOLASSES
DOUGH PRODUCTS	UNPREP MEAT/POULTRY/SEAFOOD-FRZN
DRESSINGS/SALADS/PREP FOODS-DELI	YOGURT

Notes: This table reports the product modules that we categorize as breakfast and lunch products.

Figure 12: Chain Exposure to the CEP over Time



Notes: This figure plots exposure to the Community Eligibility across retail grocery chains in the Nielsen RMS dataset. The program began in 2012, so exposure is zero in 2010 and 2011. Exposure grows dramatically in 2015 when all states adopt the program. Table 1 provides the timing of adoption across states.

Table 14: Sample Construction for Store Attrition Analysis

Initial Pool = 9,929		
Criterion:		
1.	Unbalanced RMS Panel	171
2.	Entire chain entry/exit from RMS	1,489
3.	Store switches chain affiliation	77
4.	No school match with 2010 ISP & Participation Data	1,632
Final Sample = 6,808		

Notes: This table describes the criteria that we use to create the sample of Nielsen RMS stores for our analysis of store exits in section 6.4. Most retail outlets are dropped if the entire chain exits or enters the RMS data so that we cannot observe changes in the outlet's survival or if we cannot match the retail outlet to CEP exposure.

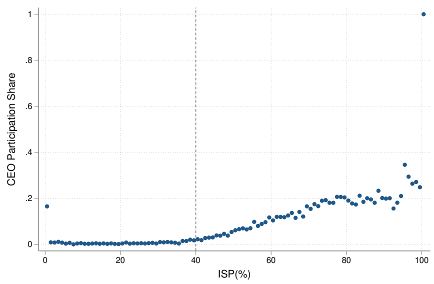
Table 15: Match Rate between NCES and State DOE Data

State	NCES and State CEP	NCES Only	State CEP Only	Total
District of Columbia	80	144	0	224
Illinois	508	7907	139	8554
Kentucky	440	2373	42	2855
Michigan	827	6218	144	7189
Ohio	4	3625	0	3629
West Virginia	249	505	34	788
Total	2108	20772	359	23239

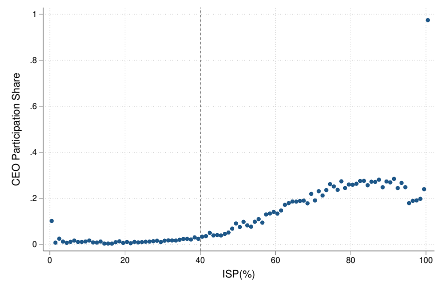
Notes: This table compares CEP participation data from the National Center for Education Statistics and the State Departments of Education. We collect this data for early-adopting states only.

Figure 13: CEP Adoption Rates over Time:
School CEP Adoption Share By Year of Adoption

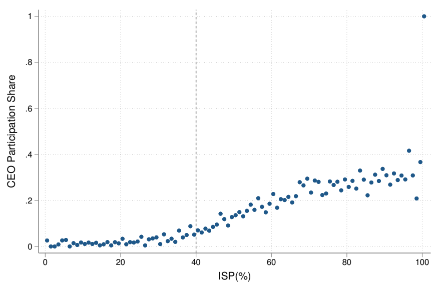
(a) Year 1



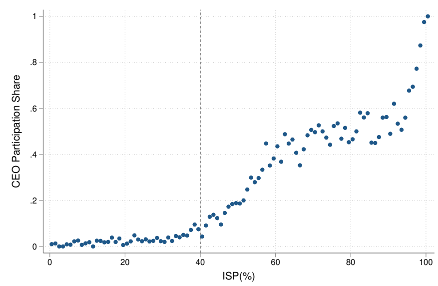
(b) Year 2



(c) Year 3



(d) Year 4



Notes: This figure plots school participation rates in the CEP by ISP and years since state adoption. Table 1 provides information on the timing of state CEP adoption.

B Robustness

B.1 Evidence on Lunches Served

An explicit goal of the CEP is to increase the number of students that eat school lunch. If the program is successful, then fewer families ought to buy ingredients at supermarkets to send with children as home-packed lunches. Thus, we estimate the effect of the CEP on lunches served in schools as additional evidence that the CEP affects demand for groceries. However, we note that CEP spillovers may be large event absent an increase in school lunches served. In equilibrium, competitors might respond to the CEP so as to maintain market share, for example, by lowering prices.

Table 16: Summary Statistics on Participation and Eligibility for Wisconsin Schools

	(1) District CEP	(2) LEA CEP	(3) Non Part	(4) School CEP
School ISP	0.682	0.679	0.219	0.545
District ISP	0.622	0.556	0.216	0.329
ADP	0.769	0.717	0.534	0.748
Observations	24	255	1749	24

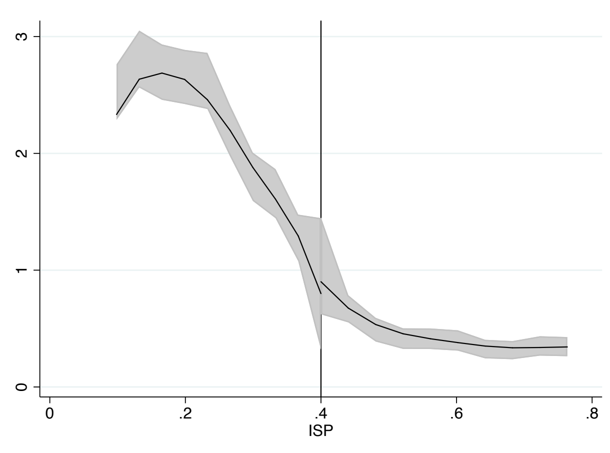
Notes: This table presents summary statistics broken out by whether/how a school participates in the CEP. For example, the sample in column 1 is the set of schools that participate in the CEP with their entire district, so that the district's ISP is the basis for qualification. Data is from Wisconsin AY 2017-2018. ADP is the Average Daily Participation rate for school lunch. The ISP cutoff is 0.4 for individual school eligibility into the CEP.

We investigate substitution patterns using school-level adoption data from Wisconsin in the 2017-2018 academic year. For each school, the Wisconsin Department of Education provided us information on ISP,[27] CEP participation, and ADP, average daily participation rate in school lunch. Table 16 provides summary statistics for this sample, broken out by CEP status. Schools participating under the CEP have higher average daily participation rates, which is consistent with a positive impact of the program. However, these schools also have higher ISPs than those that do not participate, which could drive the pattern in ADP even absent an effect of the program. Most schools participate in the CEP as part of a Local Education Agency (LEA), which describes any group of two or more schools in the same district. However, twenty-four schools choose to participate individually, which means that their individual ISP must exceed the 40% threshold. We leverage these schools to estimate

[27] The data from Wisconsin includes the true ISP, which governs eligibility for the CEP. Throughout the rest of the paper, we use the percent of free lunch eligible students as a proxy for ISP.

the causal effect of the CEP on ADP using a regression discontinuity design (RDD), following the methods outlined in Calonico et al. (2017). One concern is that schools may adjust their ISPs in order to qualify for the program; indeed, in section 4 we present evidence of bunching using a national sample of schools. In figure 14, we test for a discontinuity in the density of Wisconsin schools around 40%, but find no evidence of a bunching. We note that while the smoothness of the pdf is reassuring, the fundamental identification assumption that schools do not game ISP in Wisconsin is untestable.

Figure 14: RDDensity Plot: Wisconsin Data



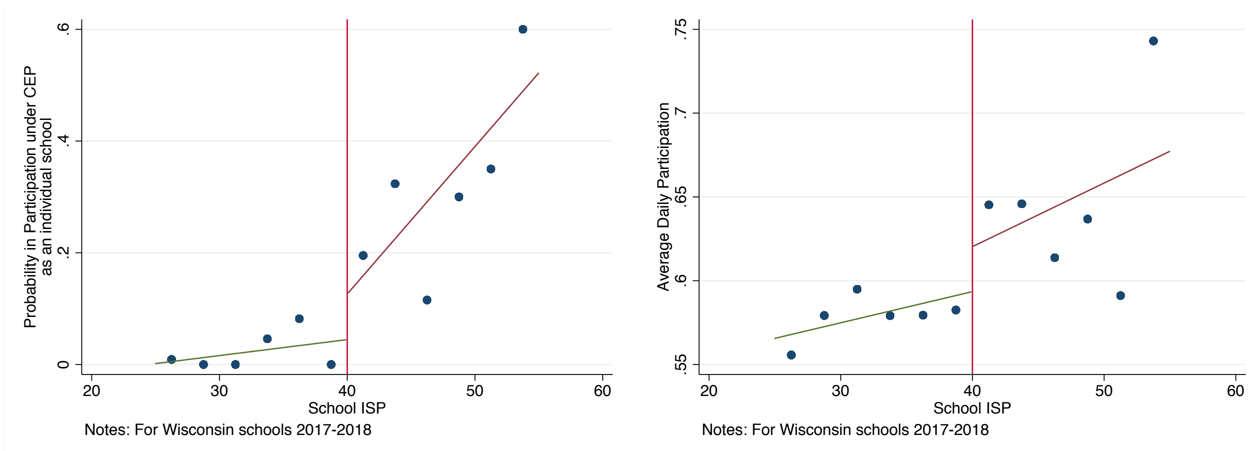
Notes: This figure plots a kernel-smoothed density of ISP rates for Wisconsin schools in the 2017/2018 AY.

We estimate the effect of the CEP on ADP using the following model:

$$y_i = \beta_0 + \beta_1 \cdot 1\left[ISP_i > 40\right] + \beta_2 \cdot ISP_i + \beta_3 \cdot ISP_i \times 1\left[ISP_i > 40\right] + \beta_4 \cdot log(E)_i + \epsilon_i \quad (11)$$

where ISP_i is the ISP of school i, $1[ISP_i > 40]$ is an indicator for whether the ISP exceeds the 40% threshold, and $log(E)_i$ is log enrollment. We are mainly interested in the coefficient, β_1, which captures any jump in the outcome variable y_i, such as ADP or CEP participation, at the discontinuity. Figure 15 presents the relationship between school ISP and CEP on the left and ADP on the right. There is a clear jump in both CEP participation and ADP rates at 40%. These patterns are mirrored in the regression results, presented in table 17. There is a 16.9 percentage point jump in the likelihood of participation under the CEP at the 40% threshold (column 2). This jump is mirrored by a 6.8 percentage point jump in ADP (column 1). These estimates imply a large impact of the CEP on lunches served; in column 3, we present the fuzzy RD estimates, which indicate that a school near the cutoff experiences a 37.5 percentage point increase in ADP when it adopts the provision. For comparison, Schwartz and Rothbart (2020) estimate that universal free lunch increases participation among non-poor students in New York City by 11 percentage points, which is within the 95% confidence interval that we estimate. While the difference is not statistically significant, the RD point estimate might be larger because it captures the increase in lunches served at marginal schools (with ISPs around 40%) rather than at the average school.

Figure 15: RD Graphs for Wisconsin



(a) Participation under the CEP

(b) Average Daily Participation in Lunch

Table 17: RD Estimates of CEP on Lunches Served

	Reduced Form	First Stage	IV
	(1)	(2)	(3)
	ADP	CEP	ADP
RD_Estimate	0.068**	0.169**	0.375*
	(0.029)	(0.070)	(0.208)
N	2053	2053	2053

Notes: This table presents results of a local polynomial regression-discontinuity design model with robust bias-corrected confidence intervals and an MSE-optimal bandwidth, estimated in Stata via the "rdrobust" command using techniques in Calonico et al. (2017). Coefficients estimate the discontinuity in ADP and CEP adoption at ISP=40% for Wisconsin AY 2017-2018.

These estimates allow us to compute the implied own-price elasticity of school lunch as follows:

$$\begin{aligned}\epsilon &= \frac{66.37}{-100 \cdot Pr\left\{P_0 \neq 0\right\} + 0 \cdot Pr\left\{P_0 = 0\right\}} \\ &= \frac{66.37}{-100 \times .6} \\ &= -1.11.\end{aligned}$$

The numerator comes from the estimated coefficient in table 17 column 3, scaled by the average daily participation at non-CEP schools with ISPs between 30 and 40%. The denominator in the average percent change in price for students at a marginally eligible school. At such a school, 40% of students already qualify for free lunch under the NSLP - for these students, there is no change in the monetary cost of lunch. For the remaining 60% of students, lunch prices fall by 100%, regardless of whether the student qualified for reduced price lunch under the traditional NSLP. The estimates imply that demand for school lunch is elastic.

B.2 Interactions with Share Children

As a robustness check, we test whether the CEP has a larger effect in communities with a higher proportion of school-aged children. The idea behind the test is that equal adoption of the CEP should have a larger effect in a state like Utah, where children comprise a relatively large share of the population (1.24 children per family in 2000) compared to West Virginia (0.72 children per family in 2000).[28] To operationalize this test, we use data from the 2010 census on the share of children for each ZIP code and interact this measure with our measure of CEP eligibility: schools with an ISP above 40% in years following their state's adoption of the provision.

Table 18: Summary Statistics on the Share of Children across ZIP codes

	mean	sd	p1	p10	p25	p50	p75	p90	p99
Zip Kid Share	0.17	0.04	0.06	0.12	0.15	0.17	0.19	0.22	0.25
N	52,916								

Notes: This table reports summary statistics for the share of the population that is under 18 years old by ZIP code from the 2010 census.

In the table below, columns 1-3 reproduce the main results on the effect of CEP eligibility on store revenue. Columns 4-6 present estimates of specification (4) with the share-kids interaction term, where the share of children is standardized. In these regressions, the coefficient on CEP adoption is large and negative, but so, too is the coefficient on the interaction term. That is, a one-standard deviation increase in the share children increases the effect of the CEP by approximately 15%. These results bear out our hypothesis that the CEP, in directly affecting households with children, has a greater effect on store revenues in ZIP codes with a greater share of children.

[28] https://www.census.gov/population/socdemo/hh-fam/tabST-F1-2000.pdf

Table 19: Effect of CEP Eligibility on Revenues in Areas with High/Low Share of Children

	(1) All	(2) B/L	(3) Lunch Meat	(4) All	(5) B/L	(6) Lunch Meat
Store Zip CEP	-0.140***	-0.155***	-0.258***	-0.141***	-0.156***	-0.260***
	(0.023)	(0.025)	(0.029)	(0.024)	(0.026)	(0.030)
CEP x Standardized Zip Kid Share				-0.025	-0.024	-0.057***
				(0.015)	(0.017)	(0.019)
F-stat	663	663	663	220	220	220
Observations	53,059	53,059	53,059	52,916	52,916	52,916

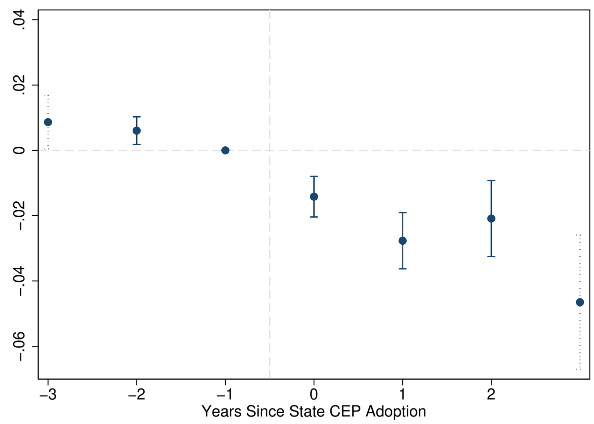
Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include a constant and store and state by year fixed effects. Standard errors are clustered at store level. Sales are aggregated to the school year, with sales from September 2014 to August 2015, for example, being associated with CEP participation in the 2014/2015 school year.

B.3 Alternative Fixed Effects

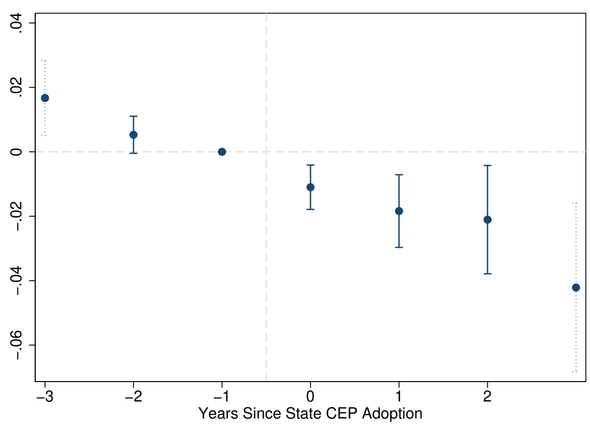
Our main regression specification (4) for estimating the effect of the CEP on store-level outcomes such as revenue and prices includes both store and county×year fixed effects. The aim of the county×year fixed effects is to capture any factors apart from the CEP that vary across time and space and influence grocery revenues. In essence, we compare within-store changes in revenue for stores near schools that adopt the CEP to stores near schools that do not adopt the CEP, but are located in the same county. In figure 16, we compare estimates across specifications where we allow for alternative fixed effects that allow for different control groups. As a baseline, subfigure 16a presents our preferred estimates. In subfigure 16b, we include state×year fixed effects, broadening the control group, but also ISP×year fixed effects that allow for different time trends for stores in high and low-income neighborhoods. In subfigure 16c, we keep the ISP×year fixed effects but narrow to the county×year fixed effects. The point estimates are fairly stable across specifications, suggesting that 2 years after the program, stores near schools that are individually eligible for the program to the CEP see a 2-4% drop in revenues. However, there is a semblance of a pre-trend in subfigure 16b. While parallel trends before the introduction of the CEP does not imply that our estimates are causal, they do suggest that the county×year fixed effects may play an important role in controlling for economic trends that vary geographically.

Figure 16: Effect of CEP Eligibility on Log Grocery Revenues with Alternative FEs

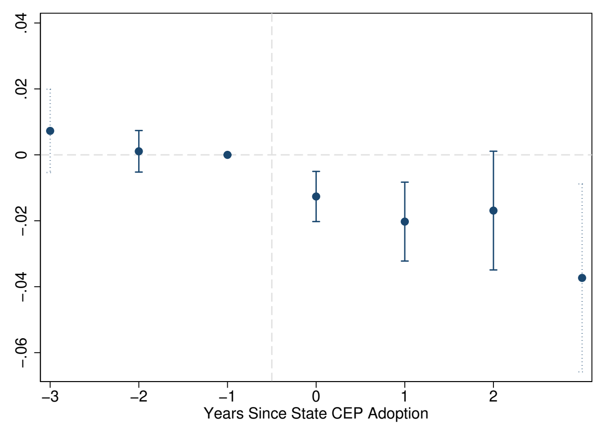
(a) Store and County×Year Fixed Effects



(b) Store and State×Year Fixed Effects + ISP40×Year FE



(c) Store and County×Year Fixed Effects + ISP40×Yr FEs

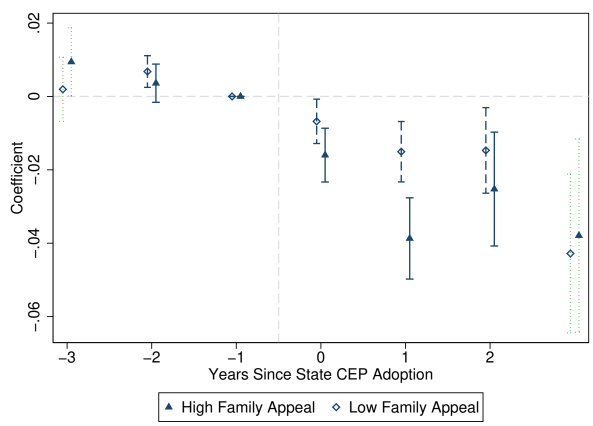


Notes: This figure plots estimates of the effect of CEP eligibility on grocery store revenues. Subfigures present estimates based on different combinations of fixed effects. Estimates with dotted confidence bands are limited to a subset of state-waves for which we observe at outcomes four years after state adoption of the CEP. The regression specification that underlies the estimates is equation 4.

B.4 Distribution across Product Groups

This appendix section explores how the impact of the CEP is distributed across product groups. In line with our finding that the direct effect of the CEP is to induce substitution by households with children away from grocery store lunch purchases to school lunch, we expect that grocery retailers experience the largest revenue declines among products that particularly appeal to households with children. To test this hypothesis, we construct a family-appeal variable, which we measure as the pre-CEP share of expenditures in a product group attributed to households with school-aged children. We then adapt our difference-in-differences specification (4) so that revenues are measured at the product group level and our explanatory variables include a triple interaction between family-appeal, state adoption, and school eligibility. Figure 17 plots an event study separately for product groups with below- and above-median family appeal. Consistent with our conjecture, revenue declines are steeper for products with above-median family appear. Note that other groups also experience a decline, which is in keeping with our evidence that households take fewer trips to grocery stores following CEP adoption.

Figure 17: Revenue Effects across Product Groups



Notes: This figure plots estimates of the effect of CEP eligibility on grocery store revenues in product groups with above and below median family appeal. Estimates with dotted confidence bands are limited to a subset of state-waves for which we observe at outcomes four years after state adoption of the CEP. The regression specification that underlies the estimates is identical to equation 4, but with each set of fixed effects (store and county-by-year) interacted with product group and the time since state CEP adoption indicators interacted with both school eligibility and an indicator for whether the 2010 share of HMS expenditure in a product group that was by households with school-age children.

B.5 Event-by-Event Analysis

In this appendix, we replicate our difference-in-differences specification (4) estimating the revenue effects by wave of state CEP adoption. In each event study, the sample only includes stores in states that adopted in the same academic year, so that the control group comprises only stores that are never-treated. As described in Cengiz et al. (2019), the virtue of this alternative specification is that it robust to treatment effect heterogeneity. The results are qualitatively similar across waves, with reduced-form revenue declines between 3-8% (the intent-to-treat effect).

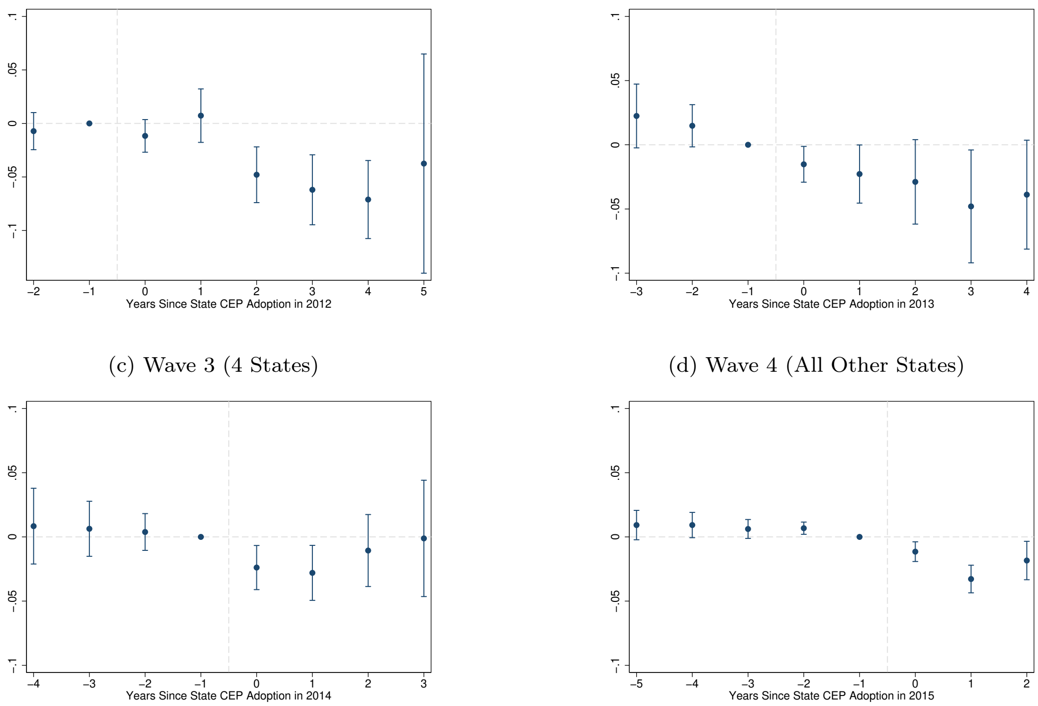
Figure 18: Revenue Effects By Year of Adoption

(a) Wave 1 (3 States)

(b) Wave 2 (3 States + DC)

(c) Wave 3 (4 States)

(d) Wave 4 (All Other States)



Notes: This figure plots estimates of the effect of CEP eligibility on grocery store revenues in sets of states that adopted the CEP program in different years. The regression specification that underlies the estimates is identical to equation 4, but with a different sample of stores in each regression. Table 1 provides information on the timing of state CEP adoption.